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AN EVALUATION AND TEST OF BIRKHOFF'S AESTHETIC MEASURE FORMULA*[1]

From the Department of Psychology of Indiana University

R. C. DAVIS

The central proposition of Birkhoff's extensive work on aesthetics (1) is that the aesthetic value of an object is measured by the ratio of its orderliness to its complexity. As he observes, this is approximately a restatement of the "unity in variety" theory of the beautiful. Birkhoff's original contributions to the theory consist chiefly in the following three developments.

1. He ventures to reduce his main thesis to exact mathematical statement as follows: $M = \frac{O}{C}$, where M is the aesthetic value, O, the order, and C, the complexity.

2. Unlike most proponents of mathematical formulation in aesthetics, Birkhoff makes a serious attempt to justify his formula psychologically.

3. Finally, he attempts to specify exactly what order and complexity are for each of a number of classes of aesthetic objects.

An important merit of Birkhoff's work is the definiteness of his propositions, which makes possible a psychological examination and empirical test as extensive as one desires. It is the purpose of the present paper, first, to inquire concerning the validity of the psychological reasoning upon which the main formula and its subsidiaries are based, and, secondly, to present the results of empirical tests of the application in two of the classes of aesthetic objects considered by Birkhoff.

PSYCHOLOGICAL PRESUPPOSITIONS

Following are the major psychological propositions used in support of the C term in Birkhoff's formula. These are either summarized or quoted from the first chapter.

1. The first phase of a typical aesthetic experience may be re-

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[1]Data herein used were collected and tabulated by Margaret Stark and Vera Kunse.

garded as "a preliminary effort of attention, which is necessary for the act of perception."

2. A state of attention consists of automatic motor adjustments consequent upon stimulation of sense organs.

3. "There is a well known feeling of effort or varying tension while the successive adjustments are being performed."

4. This effort has a negative (unpleasurable) tone.

5. This effort "increases in proportion to what we shall call the *complexity* of the object."

No experimental evidence is cited in support of these propositions. All of them are, of course, open to question. To the last, especially, there appears to be a very important psychological objection: that the author here runs astray in the stimulus-error. It is surely wrong to suppose that a one-to-one correspondence exists between complexity of stimulus-object and effort of attentional adjustment. Attending to a "simple" patch of color seems to require much greater effort than attending to the relatively complex drawing of a man. The illustration Birkhoff has chosen for his principle is unfortunate. He writes, "(in perceiving a polygonal tile) the eye follows the successive sides of the polygon and the corresponding motor adjustments are affected automatically. Nevertheless . . . there is a slight feeling of tension attendant upon each adjustment, and the complexity *C* will be measured by the number of sides of the polygon." As is well known, the eyes do not ordinarily trace the outline of a polygon, in fact, they need not move at all, during the perception of a figure.

The order *O* term is supported by the following propositions.

1. The aesthetic factor lies in "the associated ideas and their attendant feelings" to which the sensation gives rise. Sensuous feeling (feeling tone of sensation) is invariable.

2. Formal associations may be distinguished from connotative. It is the formal with which the author deals.

3. The total feeling tone is the "summational effect" of the feeling tones of the associated ideas evolved.

4. "This (summational) effect is the psychological counterpart of what we have called the order *O* of the aesthetic object inasmuch as *L, M, N,* . . . (the associations evolved) correspond to what we have termed the elements of order in the aesthetic object."

Again there is much room for dispute. Concerning the third proposition in particular there is some experimental evidence to the contrary (2). And the fourth, of course, is the stimulus-error again.

Finally, objection must be entered to term *M*. It is defined as an "index of aesthetic effectiveness." This, of course, is compounded of *O* and *C*. We should inquire, it seems, why aesthetic experiences should be regarded as compounds of these positive and negative feeling tones, and why the result of the combination should be proportional to their ratio. Concerning this last point Birkhoff quotes a "hypothetical experiment." Inevitable as the results of this experiment may seem, its outcome should be questioned, since some of the assumptions may be incorrect. (It is doubtful, for example, that $C + C = 2C$.) To demonstrate the propositions satisfactorily, actual performance of the experiment outlined and an even more heroic one on the analysis of aesthetic experience into *O* and *C* appear to be required.

Altogether we are forced to conclude that the a priori evidence in support of the aesthetic measure formula is insufficient and sometimes very dubious.

Before an empirical test of the formula can be made it becomes necessary to specify the meaning of *O* for some class of objects. Birkhoff has performed this task for a number of aesthetic classes. His method is described as "empirical," and consists in examining specimens of the class in question and deciding what features affect aesthetic feeling, giving each an estimated weight, and defining what, for that class, constitutes an "element" to be counted in the computation of *C*. An experimental test, such as the one to be described here, does not of course apply exclusively to the formula: possibly the *identification* of the elements of order and complexity may be incorrect. But if the experimental results agree with Birkhoff's application of the formula there is some presumption in its favor; if not, at least the formula is yet unprovided with empirical support.

For polygons as a class of aesthetic objects, Birkhoff makes the following identification: $O = V + E + R + HV - F$, *V* being vertical symmetry, *E*, equilibrium, *R*, rotational symmetry, *HV*, relation to a horizontal-vertical network, *F*, "unsatisfactory form." Some of these may be, for a given figure, 0, 1, 2, for others, —1, 0, and +1. The term *C* is "the number of distinct straight lines containing at least one side of the polygon." The value of *M* is obtained by summing up the positive and negative values for *O* and *C*, and obtaining the ratio. The maximum value *M* may have is 1.50, which is the value of the square. Polygons are presented with

values of —.17; presumably there might be lower values. Ninety polygons are presented by Birkhoff with the value *M* computed for each.

Empirical Evidence

Birkhoff well observes that aesthetic comparison "will have substantial meaning only when it represents the normal or average judgment of some selected group of observers." For the 90 polygons he suggests that "if there is felt to be a gradual diminution in aesthetic value as the polygons are looked at in succession . . . the formula may be regarded as justified." Using, presumably, this method, he obtained the consensus of aesthetic judgment from his classes in Columbia and Harvard Universities, finding the results in "substantial agreement with the arrangement obtained by the formula."

For a more exact test, the method of ranking was used in the present study. As it was not practicable to rank all 90 figures, ten were selected, Nos. 1, 10, 20, 30, 40, 50, 60, 70, 80, and 90, as outlined in Figure 1. Forty cards were prepared, the ten figures

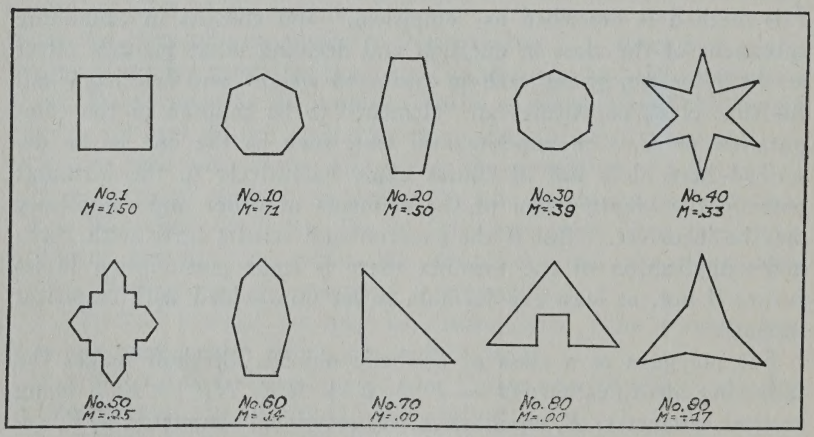

FIGURE 1
Outlines of Polygons Used

being traced from Birkhoff's plates upon each and colored with opaque blue. The order was not the same on all cards: each figure appeared in first and last place on an equal number of cards. The order of the other figures on the cards was haphazard. By this means it was hoped to avoid precedence effects. Beneath each figure a num-

ber was placed, but not the value from Birkhoff's list. Judgments were obtained from a number of university classes, 162 subjects being members of classes in psychology and 55 being members of classes in art.

Each student in a class was given a card containing the polygons and a mimeographed sheet instructing him to select first, second, third choices, and so on, and to record them by number in blanks provided.

Oral instructions were given for the subjects to judge simply on the basis of their preferences for the figures, without reference to any system of aesthetics they might know of.

In two respects this procedure with the polygons differs somewhat from that recommended by Birkhoff. In the first place, Birkhoff specifies that polygons should be placed in a vertical position. The cards were not invariably held so, but this fact would seem to be immaterial, inasmuch as the vertical position seems to play no part in Birkhoff's mathematical development. The second divergence in procedure may be more serious. Subjects were not instructed to distinguish between what Birkhoff calls the formal and the connotative associations. This seemed too difficult a distinction to make

TABLE 1

Figure	No. first choices	Weighted average rank	S.D.
A. Preferences of 162 students for the ten figures			
1	1	3.97	1.18
10	0	4.27	1.34
20	6	4.52	1.56
30	3	6.10	1.45
40	0	6.11	1.44
50	118	9.40	.98
60	29	8.42	1.18
70	2	3.47	1.33
80	3	5.64	1.37
90	0	2.94	1.16
B. Preferences of 55 art students for the ten figures			
1	1	3.93	2.97
10	0	4.62	3.25
20	0	5.61	2.41
30	0	4.47	3.05
40	0	5.40	2.24
50	47	9.52	1.54
60	5	7.90	2.01
70	2	4.00	2.02
80	0	4.64	2.73
90	0	2.93	2.14

for these figures, even if it is at all possible. Presumably, however, the "connotative association" is at a minimum in these polygons.

Results from the subjects were treated in two groups: those from the classes in psychology, as samples of college students in general, and those from the art classes, as samples of those with a special interest in design. Computations were made in two ways. According to the first method, only the number of first choices for each figure was considered. According to the second method, a credit of ten was given a figure for a first choice, nine for a second, etc., an average score and standard deviation then being computed on the basis of these scores. Table 1 shows the results for the two groups. Various correlations were then computed by the method of rank differences. These are presented in Table 2.

TABLE 2

RANK DIFFERENCE CORRELATIONS OF PREFERENCES FOR POLYGONAL FIGURES

	Average: unspecialized students	Number first choices: unspecialized students	Average: art students	Number first choices: art students
Order of Birkhoff formula	.11	—.11	.05	.00
Average: unspecialized students		.64	.90	
Number first choices: unspecialized students				.63
Average: art students				.41

It will be observed that figure No. 50, with a formula rating of .25, is by far the favorite figure in both groups by both methods of computation, and the square, with a rating of 1.5, ranks relatively low. The correlations show a moderate degree of agreement between the two methods of calculation. Of the two, probably the weighted average method is superior, because of the large number of figures which had no first choices at all. There appears to be no significant difference between the preferences of art students and those of students in general. This high correlation also indicates a high degree of reliability for the preferences. The S.D.'s indicate greater variability among the judgment of the art students. Finally,

the weighted averages show practically no correlation with the order derived by means of Birkhoff's formula.

The second type of material used for a test of the formula was poetry. For poetry, considering only the musical quality, Birkhoff defines C as the total number of "sounds" plus the number of junctures not capable of liaison. O is found by counting the number of instances of alliteration, assonance, rhyme, and musical sounds, weighting them and totaling, then subtracting weighted values for alliterative, assonantal, and consonantal excess. Meter is intentionally left out of consideration.

Birkhoff has computed the values for the ten following poetic selections.

1. Bright Star, would I were steadfast as thou art.
   (From Keats's "Last Sonnet"; $M = .62$)
2. Come into the garden, Maud,
   For the black bat, Night, has flown,
   Come into the garden, Maud,
   I am here by the gate alone;
   And the woodbine spices are wafted abroad,
   And the musk of the roses blown.
   (From Tennyson's "Maud"; $M = .77$)
3. Tell me not, in mournful numbers,
   Life is but an empty dream!—
   For the soul is dead that slumbers,
   And things are not what they seem.
   (From Longfellow's "A Psalm of Life"; $M = .73$)
4. In Xanadu did Kubla Khan
   A stately pleasure-dome decree:
   Where Alf, the sacred river, ran
   Through caverns measureless to man
   Down to a sunless sea.
   (From Coleridge's "Kubla Khan"; $M = .83$)
5. Little boy blue, come blow your horn,
   The sheep's in the meadow, the cow's in the corn.
   (From a nursery rhyme; $M = .65$)
6. He never had much to give,
   Subscription lists knew not his name,
   He was one of the many who live
   Unrecorded in charity's fame.
   (From E. A. Guest's "Contribution"; $M = .45$)
7. The white mares of the moon rush along the sky
   Beating their golden hoofs upon the glass Heavens;

The white mares of the moon are all standing on their
hind legs
Pawing at the green porcelain doors of the remote Heavens.
(From Amy Lowell's "Night Clouds"; $M = .64$)

8. Hear the sledges with the bells—
Silver bells!
What a world of merriment their melody
foretells!
(From Poe's "The Bells"; $M = .57$)

9. Take, O, take those lips away,
That so sweetly were foresworn;
And those eyes, the break of day,
Lights that do mislead the morn!
(From Shakespeare's song "Take, O, Take Those Lips
Away"; $M = .74$)

10. Onward, Christian soldiers,
Marching as to war,
With the cross of Jesus
Going on before.
(From Baring-Gould's "Onward Christian Soldiers";
$M = .51$)

He reports that these values agree in general with the previous arrangements of several observers. For a more thorough test these were mimeographed in two orders, one the reverse of the other, neither being the order of Birkhoff. Titles, authors, and M-values, of course, did not appear on the sheets. These were given to students in classes in English, one order being used for about half the students, and the other for the rest, in order to eliminate the position factor from the averages. Another mimeographed sheet with the following instructions was given.

> Read over the selections on the mimeographed sheet. Then select the one which seems to you to have the best poetic quality. Pay attention only to the sound and structure of the verse. Do not regard the meaning nor attempt to judge on the basis of metrical rules with which you may be familiar. Write the number of the best selection opposite "First choice," and then proceed similarly with the other nine selections.

Space was provided for recording the choices. Sixty-three students were used as subjects.

Results were computed as in the previous experiment and are shown in Table 3. The number of cases is hardly large enough to compute a coefficient of reliability, but a measure of the consistency of choice may be seen in the S.D.'s. These tend to be somewhat larger than those for the polygons. Between the averages and Birk-

TABLE 3
RESULTS OF JUDGMENTS ON TEN SPECIMENS OF VERSE

Selection	Number first choices	Weighted average	S.D.
1	4	5.81	1.53
2	4	5.90	2.88
3	28	8.39	2.05
4	12	7.44	2.40
5	1	3.90	2.02
6	0	4.00	2.24
7	3	3.84	2.77
8	6	6.63	2.31
9	5	6.04	2.39
10	0	3.66	1.95

Correlations:
Number first choices x weighted average .93
Number first choices x Birkhoff order .54
Weighted average x Birkhoff order .55

hoff's order there is a moderate correlation (.55), not high enough for predictive value, to be sure, but of significant magnitude. Probably the agreement here is of the same order as Birkhoff's "in substantial accord with the theory."

The result is also in agreement with expectation on the basis of practically all ideas of poetic merit, inasmuch as rhyme, assonance, alliteration, and musical sound are generally regarded as positive factors, within limits, in musical quality. Concerning the factor *C* there might, however, be more doubt. In the present experiment it serves to equate the length of the selections after a fashion. To see whether that is its principal effect other methods of equating are tried.

First, disregarding length and any other effects represented in Birkhoff's *C*, a correlation was computed between Birkhoff's *O*-values and the average preference. (The *O*-value was derived by multiplying Birkhoff's *M* by the number of sounds plus the number of non-eliding junctions. There is some error here, probably, as the writer is not certain in all cases what constitutes "a sound.") The value of this correlation (ρ) is .31. Evidently preference is more closely connected with "density" than with total effect. Dividing Birkhoff's *O*-values by the number of syllables in each selection gives a measure which correlates .56 with average preference. Finally, when the total length of the selection in centimeters is used as a denominator the values correlate to the extent of .53 with the average judgments. It appears from an empirical viewpoint that there is no inherent superiority in Birkhoff's interpretation of *C;* at least two,

and probably more, other measures of length will do as well. Preference for the original interpretation can scarcely be based upon psychological grounds, for, as psychological "elements," syllables seem much more probable than "sounds."

Summary and Conclusions

Birkhoff has attempted to derive support for his aesthetic measure formula chiefly by a priori psychological arguments. These are shown to be inacceptable, a major fault being their basis in the stimulus-error. Empirical verification of the formula is attempted in two classes of aesthetic objects: polygons and poetry in so far as its musical quality is concerned. In making empirical tests the formula cannot be applied directly, as it is too general. Some identification of elements must be made, and, for the present test, Birkhoff's identification has been accepted. The evaluation of polygons on the basis of this identification yields no correlation with the average rank assigned by subjects. For the polygons there is, therefore, no support for the formula, either a priori or empirically. It might be possible, of course, to redefine the terms so that there would be a relation between the expectations and results.

From the experimental test of selections of poetry, Birkhoff's evaluations appear to have some justification. The formula can hardly be said to be established by the moderate correlation found on the ten selections between expectation and findings; some other formula might give a better prediction. Probably, as Birkhoff suggests, the identification and weighting of elements might be improved upon. But the correlation does give an indication that the musical quality of poetry depends in part, perhaps about 25 per cent, on the amount of assonance, alliteration, rhyme, and musical sound per unit length (say per centimeter) of poetry. Naturally the results cannot be held to support the psychological principles advanced in support of the general formula, since practically any theory of poetry would lead to the same expectation.

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BASIC CONCEPTS FOR A PSYCHOLOGY OF PERSONALITY*[1]

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At the present time the greatest interference to the well-being of psychology is the separation and opposition of radically different schools of thought, each of them blind to the need for an idealogy which is general enough to include them all. The schoolmen are unwilling to acknowledge that the application of their theories is limited to the particular aspect of things which they have chosen to investigate. We are thinking particularly of the Introspectionists, the Behaviorists, and the Psychoanalysts with their respective intellectual provincialisms. These schools differ not only in their opinions as to the proper subject matter and methodology of psychology, but they are at odds—and this is the most unfortunate feature—in respect to basic theories and hypotheses. The existence of utterly different terminologies based upon different metaphysical assumptions prevents communication and understanding and tends to promote futile antagonisms. It is as with the blind men—in the fable of the blind men and the elephant—each of whom stoutly affirmed that the part of the elephant which he held in his hands—the trunk, the tail, an ear, a tusk, or a leg—was *the elephant.*

If it is true—as it appears to be—that each school has made important contributions to our knowledge of human nature, then what is required is a new scheme which will be comprehensive enough to include, as subsidiary concepts, the best of the existing formulations. The time is ripe for a unifying theory of this sort—one which will provide psychologists with a common final goal, and by so doing allow for the proper orientation of the special fields.

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[1]We have used the expression "psychology of personality" in the title out of deference to custom. It is a clumsy, tautological phrase and we propose that "personology" be substituted for it.

Definitions of Psychology

Psychology has been defined by some authors as the science of consciousness, mind, or soul, by others as the science of action or conduct, and, finally, by many modern writers as "the science of both consciousness and behavior" (2, p. 6). What is the implication? Is psychology, in fact, two sciences, each with its own terminology and conceptual scheme? At first blush it would seem so, since action, as such, is describable only in physical terms—movements in space characterized by a certain velocity and duration—and consciousness, as such, is describable only in psychical terms—images, feelings, volitions, and so forth. This impression of the incommensurability of the two kinds of phenomena commonly considered to be within the domain of psychology is reenforced by an examination of the history of methodology and theory. Introspectionism, by confining itself to the detailed analysis of conscious perceptions, has almost completely neglected behavior; whereas Behaviorism, in an attempt to be purely objective, has eliminated consciousness.

Now, to common sense, it seems that conscious processes and behavior must be related, since, to take one example, the conscious intention to make a particular movement is so invariably followed by *just* that movement. Everyday speech, indeed, by assuming causal connections between inner and outer events, is an implicit avowal of this belief. Behavior, an objective phenomenon, is explained by reference to certain desires and intentions; and perception, a subjective phenomenon, is explained by reference to physical objects. Common sense, however, is unable to justify its faith by explaining, in terms which satisfy the sophisticated, just how the two sets of phenomena are related. Thus, the mind-body problem has recurrently arisen.

Beginning at the Beginning

Though it may be some irrational bias which inclines us to agree with the currently expressed opinion that psychology should commence with the objective study of behavior, this decision seems reasonable, since people can most readily agree in their descriptions of objective phenomena and it is always well to begin at a point where agreement is possible. In studying behavior, moreover, and in attempting to construct concepts to explain it, the psychologist

aligns himself with other students of nature—physicists, chemists, and physiologists. He comes to take his proper place as a general biologist between the zoologist and the sociologist. Finally, since the enjoyable existence of living organisms is largely determined by their actions, we being interested in the former, must be interested in the latter. This initial orientation is, I believe, so correct that one should be suspicious of any scheme for representing human nature which does not give a thorough account of behavior.

The Necessity for the Formulation of Central Processes

Some Behaviorists have maintained that a science of human nature can be built upon the familiar stimulus-response formula without reference to central processes within the brain of the subject. By this simplification the investigator can maintain an utterly objective, positivistic attitude. But a man who holds this opinion is disregarding the plain fact that the actual stimulus is something which affects the brain of the subject, not something which affects his own brain; in other words, what the subject perceives is usually different from what the investigator perceives. Furthermore, it becomes clear, when the psychologist attempts to explain the simplest conditioned reflex, that central processes must be included. In the classical experiment of Pavlov, for instance, one cannot explain the dog's salivation when a bell is sounded without referring in some way to the original food stimulus with which the bell was temporally associated. Since the food, as such, is not present, and, since in describing an occurrence one cannot include factors which are not within the field, one is forced to assume the activity of an enduring impression, or *trace,* of the food within the animal's brain, and in so doing to take central processes into account. Another and more important common phenomenon which calls for the conceptualization of dominant central processes is the occurrence of long-continued single trends of activity characterized by motor movements of the most diverse sort. Since a description of the overt movements, as such, will not explain the sustained tendency—which may occur even when specific stimuli are absent—to achieve certain objective effects by one means or another, the psychologist must refer to some central directional process which persists—in

most instances, at least—until the goal is attained. The Behaviorists, by limiting themselves to the study of simple reflexes and action patterns, are able—without too gross error—to exclude consciousness and to minimize the importance of integrating or motivating cephalic processes. But, when they limit the field of study in this way, they then exclude what is most typically "psychological." Since this aspect of behavior cannot be neglected, and since it cannot be described in terms of motor movements, *qua* movements, we find ourselves compelled to attempt the formulation of the determining processes in the brain.

The judgment that one should seek an understanding of dominant cephalic processes harmonizes with the current organismic notion that behavior should be defined as the activity carried out by an organism-as-a-whole in making adjustments to its environment (12). Such a definition differentiates psychology from physiology—the science of subordinate or component organic processes functioning within an internal environment—and directs attention to the dynamisms of directional dominance. For, if the expression "organism-as-a-whole" means anything, it means that at any one moment numerous separable activities of the organism are harmoniously directed towards the realization of a single result. Such coordinated activity calls for a center of unifying control which, through lower centers, is connected with every part of the body. This center of dominance is certainly in the brain region.

Introspectionists, with the implicit assumption that the human being is a consciously perceptive mechanism, commonly commence their textbooks of psychology with an elaborate analysis of sensory processes and finish with a small, begrudgingly admitted section on action and personality. Men of this persuasion object to the practice of beginning with behavior and by their neglect of this subject dissociate themselves from the tradition of science. They seem to forget that the data which they obtain depend upon the proper motivation of their subjects, that perception is merely the first phase of a total stimulus-response event, and that the variables into which they analyze this phase have little to do with the functioning of the total organism. The Introspectionists, however, would undoubtedly agree that it is the activities which occur in the brain region that must be conceptualized by the psychologist.

The opinion of the extreme Behaviorists, on the other hand, is exactly the opposite. They emphasize action but they are usually disinclined to create hypothetical intra-cranial entities. Although physical scientists—to whom Behaviorists play flunkey—believe it is their business to introduce "fictional" and even unimaginable entities (cf. the ether) into their theories, Behaviorists think it is more scientific not to do so. By this refusal, however, they make a *rapprochement* with Introspectionism impossible, divide psychology into two distinct disciplines, and leave unformulated most of the phenomena of psychopathology as well as other subjective experiences —such as the fact of happiness. A psychology which on principle neglects the problem of happiness can never be more than an eccentric collection of dogmas.

The Regnancy

Since muscular contractions are due to the stimulation of efferent (motor) nerves, complex motor acts characterized by contractions in widely separated parts of the organism—contractions which manifest synchronous and consecutive coordination—must be the outcome of directional processes occurring in some central region of stimulation. And since the only area to and from which all nerves lead is the brain, we must conclude that the behavior of an organism functioning as-a-whole is the result of configurated excitations in the motor areas of the brain—cortical or subcortical. It is generally held, indeed, that the system which allows the organism to act as a unit is a hierarchy of controlling processes at the summit of which is a dominant integrate—here termed the *regnancy*—to which, under the best conditions, lower centers are directly or indirectly subservient. We do not know where the *regnancy* is located, beyond the fact that it is within the brain. There are reasons to suppose that it moves about the brain. It may be something in the nature of an electrical field, or electronic matrix, interpenetrating like a web the entire cephalic region, involving first one area and then another. It traverses, perhaps, the empty spaces of the brain. But, wherever it may be located, the proper aim of the psychologist is the conceptualization of its constitution. The regnancy may, at this point, be defined as the organized aggregate of mutually dependent processes which momentarily determines the functioning of the or-

ganism "as a unit." Thus, the regnancy dominates the activity of the organism when this activity has a "total" character. The principles of Gestalt psychology are applicable here, since, when the organism is functioning economically, one cannot conceive of a regnancy except as a temporally configurated whole. The conjunctivity of regnancy, however, should not be overemphasized, since there are many states—conflict, emotional panic, dissociation, schizophrenia—which are undoubtedly accompanied by division and disorganization of the central processes. For the sake of clarity, however, it is best to begin by considering only integrated (conjunctive) regnancies.

So far we have spoken only of the motor phase of the regnancy, but it must be obvious that the sensory phase is an equally important part of it. Furthermore, the regnancy includes integrative (cognitive) and emotive (affective) processes, all of which may be regarded as electrical in nature. Finally, since the processes of the regnancy occur within or through a fluid medium, to explain certain states one must take account of the chemical constitution of the blood and lymph in the cephalic region. The importance of this factor is suggested by what is known about the effect upon cerebral activity of internal secretions, drugs, anaesthetics, and changes in oxygen tension.

Since the regnancy differs from moment to moment, one must conceive of a person's life as a temporal series of regnancies, each of which—though related in various ways to preceding and succeeding regnancies—is unique. This is a conception which stresses dominant functional activity—the most actual *thing* being a single regnancy. As Whitehead puts it:

> In speaking of any actual individual, such as a human being, we must mean that man in one occasion of his experience. Such an occasion, or act, is complex and therefore capable of analysis into phases and other components. It is the most concrete actual entity, and the life of man from birth to death is a historic route of such occasions. These concrete moments are bound together into one society by a partial identity of form, and by the peculiarly full summation of its predecessors which each moment of the life-history gathers into itself. The man-at-one-moment concentrates in himself the color of his own past, and he is the issue of it. The "man in his whole life

> history" is an abstraction compared to the "man in one such moment" (13, p. 27).

According to our conception the processes which go to make up personality should be "placed" in the brain. Personality is not the constitution of the entire body. Though we know that various physiological activities may modify personality profoundly they are, strictly speaking, "outside" of personality. They belong to its *internal* environment just as war, which may also modify personality, belongs to its *external* environment. This view is in direct contrast to Menninger's notion that personality "means the individual as a whole, his height and weight and loves and hates and blood-pressure and reflexes; his smiles and hopes and bowed legs and enlarged tonsils. It means all that anyone is and all that he is trying to become" (7).

The Regnancy and Consciousness: The Double-Aspect Theory

The regnancy is by definition the momentary cephalic process which is initiated by incoming excitations (from within and without the body) and terminated (as a rule) by outgoing activations. Sensory and motor processes are said to be regnant when it can be shown that they influence the course of the total organism.

From this it would seem that there must be a close relationship between the regnancy and a moment of conscious experience. For everyday experience and the experiments of psychophysicists have demonstrated that under ordinary circumstances there is an absolute correlation between the sufficient stimulation of sensory end-organs and the subjective experience of perceiving, as well as between the subjective experience of conation (striving) and the overt movements of the body. It is the verdict of experience, moreover, that in many instances particular bodily movements are the necessary outcome of particular perceptions. Thus, perception seems to be correlated with incoming regnant excitations and conation seems to be correlated with outgoing regnant activations. Anybody can perform this experiment: strive to flex the right thumb and observe the result. Under average conditions there will be an invariable temporal relationship between the awareness of striving and the movement of the thumb. This experiment—which is as valid as

any that can be done in science—leads to the conclusion that striving is coexistential with activations at the motor pole of the regnancy.

These and other such facts suggest the conclusion that consciousness is an attribute of at least some regnant processes, and that the determining seat of consciousness is in the brain—a conclusion which is reinforced by a mass of data showing the close relationship between changes in the brain and changes in conscious states. Thus, we arrive at the proposition that subjective realities and objective realities—that is to say, physical processes in the brain which would be objective realities to an experimenter, were he able to observe them—are somehow intimately related.

The best and most widely accepted solution of the mind-body dilemma is the double-aspect hypothesis. This states that a conscious experience and certain physical processes in the brain are simply two aspects of one actual event. The former represents the inner or subjective aspect—how the process appears to itself—and the latter represents the outer or objective aspect—how the process, if it could be observed, would appear to others. All mental activity, according to this notion, is associated with physical activity—let us say of an electronic nature. A change in one signifies a change in the other. This proposition, though it can neither be proved nor disproved, seems to be the most convenient working hypothesis for a psychologist to adopt. It is useful because it establishes a theoretical ground for the construction of a conceptual scheme which both groups—Introspectionists and Behaviorists alike—should be able to accept.

Unconscious Regnant Processes

Now, the double-aspect hypothesis states that every conscious process is the subjective aspect of some regnant process, but it does not state that every regnant process has a conscious correlate. It appears, indeed, that to explain any conscious event as well as to explain any behavioral event one must take account of more variables than those which are at the moment present in consciousness. "Regarded as events," Köhler points out, "the facts and sequences of our direct experience do not, taken by themselves, represent complete wholes. They are merely parts of larger functional con-

texts" (4). The following examples, some of which are taken from Köhler, support this opinion:

1. The perception of the "Dipper" is an immediate experience in which the form is given as-a-whole. The stars are not organized into this common shape by a conscious process. It comes to us ready made. Presumably there have been previous impressions of actual dippers which have left traces in the brain, and in the present act of perception some interaction between the memory image of a dipper and the impression from the heavens has occurred. But this memory image is not in consciousness.

2. In the recognition of a person whom we have met once and not seen for a long time, we are frequently conscious of the interaction between the memory image and the present impression. But later, after frequent encounters, immediate recognition occurs. On such occasions, though the memory image is not in consciousness, to explain the recognition we must suppose that it is still functioning.

3. When of an evening I am conversing with a friend, I am reacting from moment to moment on the basis of a great many recognitions and suppositions which are not in consciousness. For instance, that the floor stretches out behind me—I should be anxious if there were a yawning chasm behind my chair—that I will be free to leave at a certain hour, and so forth. Such assumptions, though not conscious, are providing a time-space frame for conscious events and hence are determining their course.

4. One may pass a man in the street and immediately think: he appears anxious, as if he were about to face some ordeal. The conscious perception of the man's face as a physical schema, however, may have been so indefinite that one is utterly unable to describe the features which contributed to the apperception of his inner state.

5. When one is learning to drive an automobile, one is, at first, aware of every accessory intention and subsequent motor movement, but later, when proficiency has been attained, the details of the activity are seldom in consciousness. We must suppose, nevertheless, that ordered activations are occurring at the motor pole of the regnancy.

6. Absent-minded acts which involve movements of the body as-

a-whole are performed without awareness of conations similar to those which usually precede such actions.

7. When a man is building a house, let us say, he is usually conscious from moment to moment of his intention to realize a particular subsidiary effect. Though the idea of the major effect—the image of the completely constructed house—is not in consciousness, it must be active, since each conscious conation and movement is so obviously subservient to the underlying total purpose.

These examples point to the fact that the extent of the regnancy is greater than the extent of consciousness. It is as if consciousness were the illumined region of the regnancy; as if a spotlight of varying dimensions moved about the brain, revealing first one and then another section of successive, functionally-related regnancies. The examples demonstrate, furthermore, that, since a conscious experience depends upon interrelated extra-conscious variables, it can be understood only when it is viewed as part of a larger whole. Thus, to explain a conscious event, as well as to explain a behavioral event, all the major variables of the regnancy must be known. According to this conception, then, the goal of the Introspectionists and the goal of the Behaviorists becomes the same—to determine the constitution of significant regnancies. To agree about this matter, however, the Introspectionists must accept the theory of unconscious regnant processes, and the Behaviorists must attempt, as physicists, chemists, and biologists have attempted, to conceptualize the phenomena which underlie appearances.

Variables of the Regnancy: The Problem of Terminology

According to the double-aspect theory, if one group of experimenters tried to arrive at the constitution of the regnancy by studying behavior, and another group by studying the presentations of consciousness, the final outcome—if both attempts were successful—would be two sets of exactly equal equations, one set composed of subjectively derived symbols and the other composed of objectively derived symbols. For convenience, one or the other could be selected, and, thenceforth, it would be understood that each symbol (component, attribute, or dimension) represented *both* the subjective and the objective aspect of a single variable. Since the chances of determin-

ing the constitution of the regnancy are greatly enhanced if objective behavioral data are contemporaneously supplemented by adequate subjective reports—that is to say, if the investigator maintains the two points of view at once—and since a single set of variables, descriptive or explanatory of both consciousness and behavior, is the desired result, it might be better to decide ahead of time whether we should attempt to pattern our hypothetical formulae upon the existing objective (physical or physiological) constructs or upon the existing subjective (psychological) constructs.

The student of behavior has for his data the records of the physical movements of organisms and the objective effects which they achieve. He starts, in other words, in the domain of physics; and so, when he attempts to develop explanatory concepts for what he has observed, he turns, quite naturally, to the facts and theories of neurophysiology. Now, it happens that neurophysiologists have discovered that the mechanistic theories handed down to them by the physicists of the nineteenth century are quite adequate to explain the characteristics of the isolated organic processes—such, for instance, as the nerve impulse—which they have made it their business to investigate. Consequently, the psychologist may be inclined to adopt the language of neuroanatomy and of neurophysiology, and, believing himself firmly grounded upon the fundamental sciences, construct a mechanistic scheme to explain what happens between the time the stimulus is received and the response given. Thus, he may come to talk about stable nerve connections, synaptic facilitation, sensitization of nerve paths, chronactic indices, and so forth. Facts of this sort are undoubtedly important, but, by referring to them alone, one is utterly unable to account for the novel, complex, adaptive, motor, and verbal acts of human subjects.

Neurophysiologists have not yet attempted the almost impossible task of studying the physical aspect of regnant processes, though these were suggested as worthy of investigation as long ago as 1879 by St. George Mivart:

> But as each living creature is a highly complex unity—both a unity of body and also a unity of force, or a synthesis of activities—it seems to me that we require a distinct kind of physiology to be devoted to the investigation of such syntheses of activities as exist in each kind of living creature. I mean to

> say that just as we have a physiology devoted to the several activities of the several organs, so we need a physiology specially directed to the physiology of the living body considered as one whole, that is, to the power which is the function, so to speak, of that whole, and of which the whole body in its totality is the organ. In a word, we need a physiology of the individual (8, p. 223).

If neurophysiologists turned their attention to the study of regnant processes, they would soon discover what modern physicists have demonstrated when dealing with their own data, namely, that mechanistic theories are not applicable. But, whether this is strictly true or not, the fact remains that no adequate physical concepts to describe the regnancy have been devised by Behaviorists, physiologists, or physicists. And so we must turn to the presentations of consciousness and consider whether the entities identified by introspection are adequate for our purpose.

Though it is usually impossible, as I have pointed out, to explain either a conscious experience or a behavioral event by limiting oneself to the elements which were, at the moment in question, within the field of awareness, it seems to be possible for fully conscious human beings, under favorable conditions, by giving attention now to one, now to another aspect of the total situation, to bring to mind most of the factors necessary for an explanation of the original event. When there is a clear presentation of the stimulus situation and there is time for the perception of the internal sensations, affections, and evoked images, a trained introspectionist may usually recognize the processes which are engendering the inclination towards a certain response. We suppose that what happens under such conditions is that elements come to the focus of attention which were just previously active though unconscious.

At this point, a much debated question presents itself: If at one moment a variable—let us say the image of a piece of food (unconditioned stimulus)—is conscious and therefore regnant, and at the next moment is unconscious though still regnant—because it causes salivation—what term shall we apply to it at the second of these two moments? There are some men who have argued that the word "image"—as well as every other consciously derived variable—applies to an element in consciousness, and that to use the term for

something that is unconscious is to commit a logical fallacy. They favor the use of a term which refers to a physical entity in the brain. I find it impossible to agree with this conclusion for the following reasons:

1. By performing the classical conditioning experiment of Pavlov (bell-food-salivation) on a human subject, it may be demonstrated that the impression of the formerly perceived piece of food functions in exactly the same way—that is, it excites salivation—when it has a subjective correlate (conscious image of food), as it does when it has no subjective correlate. It is evidently the same variable in both events, and hence, if it is called an image in one instance, it should be called an image in the other—though in the latter event it should be termed an unconscious image.

2. According to the double-aspect theory, a conscious image is coexistent with a physical process. We can hardly imagine what the nature of such a physical process may be, but, for the moment, let us call it X. The image is the conscious subjective aspect of a process, the objective aspect of which is X. If this is true, it is admissible, when referring to this process, to use either term. Thus, even when X has no subjective correlate one may speak of it as an image—in this case an unconscious image.

3. The psychologists who object to this termiology would use the term "image" when X develops a subjective aspect, and some such term as neurogram when X is unconscious. We object to such a practice, however, on the grounds that one should avoid mixing symbols which refer to the subjective aspect of events with those which refer to the physical aspect. One should speak of either a neurogram throughout or an image throughout.

4. We have no way to describe the interacting trace of a previously perceived object, such as a piece of food, except in terms of organized sense perceptions. These give us objects in settings, or figures on grounds. If the impression left by a former precept is functioning unconsciously, it must still be represented in perceptual terms, for we have no others. Therefore, even if we should prefer to describe this unconscious factor by a term which refers to some hypothetical neurological entity—a neurogram, let us say—we should have to speak of it as a neurogram of a certain object in a certain setting. This would be even more illogical than referring to an unconscious

image, since it is hardly conceivable that by examining the brain one could observe there photographic replicas of previously observed objects. We shall probably never wish to substitute for perceived "wholes"—chairs and tables—an account of the physiological processes engendered by them.

5. Since any concepts which can be developed to describe unconscious regnant processes must necessarily be hypothetical (convenient fictions), it is scientifically permissible to imagine such processes as having the properties of conscious processes, if, by so doing, we provide the most reasonable interpretation of the observed facts. That the theory of unconscious psychic processes has great resolving power becomes apparent when one applies it to the heretofore mysterious phenomena of psychopathology.

The conclusion at which we arrive is this: in formulating regnant phenomena, constructs derived from subjective experience are at present more useful than those derived from physiological investigations. Consequently, we propose that the former be employed, and, for the sake of consistency, be employed throughout. Since it is proper to begin with the study of behavior—the behavior of organisms which cannot verbalize their subjective experiences as well as those that can—one must so define the regnant variables that their occurrence may be inferred on the basis of objective data alone. Thus, such symbols as "perception," "image," "conation" may be used to refer to hypothetical physical processes—the nature of which may or may not be known—and, if there is sufficient objective evidence, they may be used whether or not the processes for which they stand are accompanied by consciousness. MacCurdy, whose excellent book, *Common Principles in Psychology and Physiology,* has not been accorded sufficient attention in this country, uses the term "image," or "imaginal process," in this way. MacCurdy's definition is as follows:

> An imaginal process, from the standpoint of an objective observer, is some kind of a reproduction of a specific bit of past sensory experience, which is inferred to exist from the presence of a reaction for which the specific experience would be the appropriate stimulus—this reaction not being completely accounted for by any demonstrable environmental event (6, p. 14).

Adopting a somewhat similar point of view, Tolman (11) uses "cognition," and other subjectively derived terms, to stand for separable determinants of behavior (regnant processes)—the activity of each of which is manifested by some particular behavior characteristic. Thus, everything that we have said about this problem of terminology is merely confirming MacCurdy and Tolman.

The physical aspects of most variables are extremely hypothetical. For, though there is considerable knowledge of the correlates of sensory experience—knowledge which Boring (3) has so succinctly summarized—the psychophysics of efferent activations remains a relatively undeveloped field.

Event and Thema

Psychologists have always agreed that conscious experience should be analyzed into separable variables (components, attributes, or relations). But, it is only now that some are becoming convinced not only that each of the selected variables must be considered to have an objective aspect, but also that one or more of these variables may at any time be unconscious—that is, it may determine a conscious experience or a course of action without the subject's being aware of it. Gestalt psychologists have taught us, furthermore, that our variables are not dependent entities. They are mutually dependent parts of configurated wholes, which the psychologist abstracts by analysis, and later mentally resynthesizes in order to arrive at an imaginative representation of the original totality. Though one should not exaggerate the unity of regnancies, it is best to begin by assuming that each of them is an ordered whole. We must remember that, if there were no organization, there would be no organism.

To accord with the philosophy of organism as developed by Whitehead (14), a regnancy, I believe, should be regarded as a single, complex temporal gestalt, a "drop of experience," to use William James's phrase, which consumes, let us say, a small fraction of a second. It is one kind of "actual entity"; for a psychologist the most "real thing." Unfortunately, we are unable to observe, subjectively or objectively, the manifestations of a single regnancy. Just as perception is unable to distinguish what the new cameras reveal—for instance, the separate flashes of a single bolt of lightning—so is introspection, because of its limited span and speed of discrete aware-

ness, unable to grasp a single regnant moment and hold it for analysis. From numberless, successive regnancies one apprehends only fragments and much of this is immediately forgotten. Consequently, one cannot deal directly with the ultimate organic units.

It seems, however, that, since successive acts, as well as successive moments of subjective experience, frequently manifest an obvious degree of functional coordination, successive regnancies must be bound together into larger temporal wholes; and, if this is true, these may be selected as our fundamental psychological units. We have only to decide upon the best criterion for distinguishing separate, single, temporal wholes. But, before attempting to do this, we should ask ourselves: what is it that organizes a single regnancy or a succession of regnancies?

The expression "functional organization" is used when significant effects are economically achieved by the harmonious coordination of a number of different parts. In the simplest case several mutually dependent processes contribute to the achievement of a single result. This is not to be taken as a manifestation of conscious purpose, but as a manifestation of a tendency towards temporary end states. It is similar to the phenomena described by the Second Law of Thermodynamics. Thus, since organization always involves a directional tendency, that is to say, a tendency to achieve certain effects, and, since it is this tendency which unifies diversities of function, a succession of regnancies may be taken as a single temporal gestalt when the same tendency recurs in (or perseverates through) each of them. According to the terminology which we have adopted, the regnant directional tendency is the immediate result of a *need*—a *need* being defined as a regnant tension which is evoked by the perception (conscious or unconscious) of a certain kind of internal state or external situation. The tension tends to persist and to incite effective activity until an internal state or external situation (opposite in kind to the one which stimulated it) has been arrived at. Thus, as long as a single need persists, successive regnancies will be more or less unified. Even when nothing is achieved—due to the organism's ineptitude—there is, at least, a perseveration of directional effort. The exposition of the concept of need must be postponed, but enough has been said about it, I think, to suggest the following proposition: since the organization of successive regnancies into one society is due to

the operation of a single need, the two limits of such a society—a distinct, temporal gestalt—are established by the arousing and stilling, respectively, of that need. Thus, other things being equal, degree of regnant integration is a function of the intensity and protensity of an uncontested need. Since, according to our formulation, effective activity is the direct outcome of a need, we may regard the latter as the underlying, motivating force of the organism. A *mode* (action pattern)—a subordinate concept—is the means whereby a need is stilled (equilibrium is restored, the organism is "satisfied"). It is important to distinguish between these two, since approximately similar effects (need-satisfactions) are achieved by different modes, and approximately similar modes may lead to different effects.

A primary (somagenic) need is the direct resultant of the internal perception (conscious or unconscious) of a specific bodily state. This state may be aroused spontaneously, that is, it may be the product of a succession of physiological events, or it may—during its *ready* period—be excited by the (external) perception of appropriate incentives. In most cases we should start with the internally evoked need rather than with the external stimulus, since (1) self-generated activity is the primary "givenness" of all living organisms, (2) a need may be internally generated in the absence of external objects—the organism may search for food, (3) the selective perception of some rather than other external incentives is due to the operation of a particular need, and (4) even an unconditioned external incentive does not evoke specific regnant tension unless the need in question is in a state of readiness—a replete organism does not respond to food. Since we know, however, that the organism usually has many potential needs in readiness, which one of these is stimulated will be decided by what incentives are present in the environment. Furthermore, the characterization of the incentive which arouses or is sought by the need serves as a further characterization of the need itself. Finally, we know that the frequency, the characteristic intensity and protensity, as well as the integration of recurrent needs may be temporarily or permanently modified by manipulating the environment. Consequently, the discovery of possible stimulus-response patterns is a matter of prime importance.

The special task of the student of motivation, then, becomes the discovery of all the separable variables (components, attributes, and

dimensions) which may combine to determine the trend of behavior as well as those which may combine to determine the specific modes employed. As Lewin puts it: "The *cause* of the events is the *relationship between the parts of the situation as dynamical facts,* and a complete characterization of these dynamical facts would be a complete analysis" (5). It may be suggested that this is the major problem of general psychology.

To accord with these considerations the fundamental dynamic unit might be said to consist of a single stimulus-response pattern. Unfortunately this convenient terminology does not suit us, since the term "stimulus" usually stands for a single excitation, and "response" for a single reflexive contraction. In our dynamic unit the stimulus is not to be understood as a single sense-impression, structured or unstructured. For, just as we have argued that the fundamental response is not the immediate specific act, *qua* act, but the trend of activity as manifested by the succession of subsidiary effects achieved by many different acts, so also do we argue that stimuli usually appear objectively as an ordered succession of related impressions, that is, as a stimulus-gestalt which may be taken as-a-whole. The successive single stimuli are organized as a totality either because they "mean" the same thing or because they represent different aspects of the same thing—they may be words, for instance, spoken by an object, words which manifest a single, persisting attitude—or because they represent harbingers, agency objects, or pathways prospective of certain consequences if approached, manipulated, avoided, or otherwise responded to. For such a stimulus-gestalt we shall employ the term press.[2]

Thus, instead of using the phrase "stimulus-response pattern" we shall refer to a "press-need (p-n) pattern."[3] The stimulus-response pattern is adequate for reflexes (fixed relations), but when, as is usually the case, it is a matter of complex behavior, we must

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[2]As a plural form we select "press" (rather than "presses").

[3]A need may be either exopsychic—directly achieving physical or social effects in the environment—or endopsychic—achieving internal psychical effects (such as creating imaginal or verbal reconstructions of emotional experience, solving intellectual problems, arriving at judgments of value, constructing conceptual schemes, resisting temptation, developing will power, and so forth). In the present discussion, however, I have limited myself to the consideration of exopsychic needs, and I shall continue to do so.

use the p-n formula. As a rule, a p-n occurrence may be analyzed into numberless subsidiary stimulus-response connections, but what is important to note is that the latter will usually differ according to the nature of the press-need relations, a fact which proves the subsidiary dependence of the former.

Thus, the limits of a temporal gestalt are usually set by the duration of the press-need combination rather than by the duration of the need alone. A succession of regnancies which forms a temporal gestalt of this kind—a complex series of processes co-ordinated and limited by the endurance of the combination of a single press (dynamically related series of perceptions) and a single need (regnant directional tension)—will be called an *event.* Since the perceptions of the subject usually correspond to the perceptions of the investigator, and since the activity of exopsychic needs is usually manifested by a particular trend of overt action, the word *event* may be conveniently, though loosely, used to designate an objective occurrence or episode—for instance a single subject-object interaction. An event, then, is any actual, concrete occurrence which has a simple p-n structure. Since there are many possible presses and many needs, there are an even greater number of press-need combinations (kinds of events). The term which has been selected to designate the particular press-need combination is *thema—thema* being defined as the dynamical (p-n) structure of an event. A *thema* also includes a reference to the outcome of the subject-object interaction, expressed in terms of degrees of satisfaction or dissatisfaction (success or failure) for the subject. Thus, a person's life may be dynamically formulated as a succession of more or less interrelated *themas*—some of them recurrent and typical, others occasional and critical.

When nothing is known of the press, or the press is merely the usual environment, the *thema* will consist merely of the subject's initiating need. When, on the other hand, only the press is known or the press is of sole importance—something happens, or an object does something and the subject merely experiences it or adapts himself to it—the *thema* is constituted by the press alone. In representing an event, the symbol "p" is used as a prefix before the word which characterizes the environmental press (incitement or reaction), but no symbol is attached to the word which characterizes the subject's need (initiation or response). In events which involve two human

beings the press may be appropriately described in terms of the object's action (manifest need). Thus: Robbery → pPunishment →Atonement th signifies that the subject stole something, was punished by the object, and then, later, atoned for his act. Events which are merely phantasied or dreamt are differentiated by enclosing the *thema* within square brackets and putting the abbreviation "ph" before them: ph [pPursuit (Monster) → Flight → pCapture]. It is a great mistake to suppose that one can arrive at a satisfactory understanding of personality by limiting oneself to the observation of overt behavior. Inner states are facts of experience and as much a part of personality as overt actions. Our own procedure is to make a distinction between *latent* and *manifest* aspects of personality.

The press is named in terms of its common *signification*—its usual, socially-accepted (objective) meaning. In this context "meaning" refers to the "bearing" of the press upon the enjoyable existence of most subjects. The meaning which the subject gives the press is indicated by his response. It may be a special, private (subjective) meaning. If, in any instance, it is necessary to represent the strength of a press or the force of a need, this may be done by the use of a number [from a 0 (zero) to 5 (five) scale] placed after the term, thus: pDanger 2, or Aggression 4, and so forth.

Analysis and Classification of Events

If personology is ever to become a systematic discipline, it must begin with an analytical description and classification of its subject matter. If, as we have suggested, psychological events form the subject matter of personology, then it is these which must be analyzed and classified. This is a methodological step which other sciences—chemistry, zoology, and botany—have had to take, and there is no reason to believe that personology can avoid it. If this opinion is correct, we must analytically describe events and then mentally separate those which are dissimilar and aggregate those which are similar. Each aggregate will manifest a uniformity—a uniformity which must then be defined and named. This will form the basis of a classification.

Each event may be analyzed (described) in a variety of ways. It depends upon what kinds of attributes, or variables, are shown to be significant. The mode of analysis which we have suggested

—by which an event is differentiated in terms of a particular press-need combination—may be termed *thematical.* What could be called a *structural* analysis would distinguish and measure such general variables as reactivity (speed of response), tempo (rate of functioning), intensity (force of response), protensity (endurance of response), conjunctivity (coodination of response), and so forth. Together with "emotionality" these are considered by some psychologists to be variables of *temperament* [F. H. Allport (1), Bloor, and others]. McDougall, on the other hand, calls them variables of *temper.* There are other modes of analysis, some of which will have to be simultaneously employed in order to obtain an adequate representation of a personality, but this is a matter which falls outside the compass of present considerations.

The Order of Personological Variables

It will be noted that a dynamical analysis yields variables of a high order—a kind of press and a kind of need, each of which is necessarily a rather ill-defined, complex, abstract entity. For in an actual event the press is the product of innumerable, nameless low-order variables, and the need is objectified by action patterns which may be analyzed into countless contributing modes. A concrete press, for instance, might be the sudden encountering in the wilderness of a grizzly bear facing the subject a few feet away—the subject being on foot and without a gun. This might be designated "pDanger (Wild Animal)." If the encounter were less sudden, if it occurred near a convenient retreat, if the bear were tame or a cub or a black bear or chained or in a cage or a long distance away or looking in the opposite direction, if the man were armed or in an automobile, the press might be different. It might no longer be "pDanger." Likewise, the response to pDanger might be the need for Security, but what one would actually observe would be a man climbing a tree—an action which could be analyzed into a vast number of extensions, flexions, rotations, abductions, and adductions of both limbs.

It is the business of general psychology to study these low-order variables. They are important determining factors, but they do not fall within the domain of personology.[4] If personology stopped

[4]General Psychology attempts to discover the changes in the group (or

to analyze every event into its molecular parts, it would never get on with its proper business—the representation of personality as-a-whole. It is a general principle in science that the subject matter of each discipline demands concepts which are directly applicable to its own level of complexity. The concrete phenomena should be immediately illumined by the variables employed. Any amount of analysis is permissible, but it must be shown how the low-order entities thus disclosed were combined to form the high-order entities originally distinguished. In applying this principle to the matter at hand, we should say that the personologist has to deal with the reception in the regnancy of a pattern of afferent processes originating in a press and the excitation there of a pattern of efferent processes to form a trend, each pattern taken as a single variable. It is the psychophysicist or psychophysiologist who busies himself with the details of sensory and motor processes. The variables of the personologist are complex products of numberless determinants, which is one of the many reasons why they can never be more than inaccurate generalities.

The Representation of Personality

If the *thematical* mode of analysis can be made efficient and the most important *themas* are defined and named, it may be feasible abstractly to represent a person's life in terms of successive *themas.* This abstract biography, or *psychograph,* would resemble a musical score. It would show what differentiations and developments occurred with age and also what *themas* were most frequently repeated during any particular period. Presumably such a graph would have a recognizable time-structure. For just as each event is organized within itself, so also are successive events organized in respect to each other—at least to some extent.

These considerations suggest—as Whitehead (13) has pointed out—that there are three possible meanings for the notion of a particular personality: (1) the configuration of the regnancies during

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majority) response which occur when the environment is systematically modified, whereas Differential Psychology (McDougall's term), or Personology (our term), takes note of the differences between individual responses to the same environment. It must be obvious that the two points of view are complementary to one another.

one event in a subject's life—the most concrete meaning, (2) the historic route of such configurations from birth to death, and (3) the most commonly repeated configurations during any period of his existence. These may be named: momentary personality, life-time personality, and common personality, respectively. The latter, which is the most abstract, is the one which psychologists usually have in mind when they use the term "personality."

A psychograph of a life-time personality may be greatly abbreviated by omitting the *themas* which recur in the everyday experience of all people. This procedure would be in accord with definitions of personality based upon idiosyncrasy: Personality "consists in those attributes which differentiate it from other individuals" (9), is that "combination of behaviour forms in the individual . . . which distinguish that individual from others of a group" (15), is "the organized system, the functioning whole or unity, of habits, dispositions and sentiments that mark off any one member of a group as being different from any other member of the same group" (10).

As important as the recognition of simple and complex *themas* is the recognition of the change of *themas* and the change of thematic relations with age. This involves a *developmental analysis,* which calls for a separate set of variables.

Common experience and research have shown that many of the press in a person's life are not merely thrust upon him. They are searched for or selected. Dominant needs drive him towards certain situations and away from others. There is, in other words, a phantasy, plan, or purpose determining his activity which includes imaginal representations (conscious or unconscious) of the desired or undesired objects. Thus, an inner tension is usually combined with a temporal gestalt of images. Together they constitute what may be called a need-integrate. Since a need-integrate is a phantasied, wished-for event, it may be analyzed thematically. Thus, we may say that a person is motivated (consciously or unconsciously) by one or more need-*themas*. With these considerations in mind, a much briefer formulation of a personality may be arrived at if it is found that certain simple or complex *themas* recur frequently and that these may be explained by assuming the activity of an underlying need-*thema*. A widely and persistently determining need-*thema* of this kind may be called a *unity-thema*. A *unity-thema* is

usually found to be the repetition of or a reaction formation to some infantile experience—usually an excessive gratification, frustration, deprivation, or trauma.

It is often illuminating to portray the life of a subject in terms of his interactions with objects of special interest. A single meeting, or a series of recurring associations between a subject and a particular object (a thing, an animal, a human being, or an institution), may be called a *relational.* Since an enduring relational may be represented as a sequence of constituent *themas,* relationals are convenient foci for the organization of thematical material.

This, in brief, is our tentatively adopted procedure for the analysis and representation of the dynamical constitution of a human being. To those who complain that there are hundreds of distinguishable *themas,* and to describe them all would be a Herculean task, our answer would be that until the task is performed the science of personology will be adrift.

Summary

In the present communication I have suggested that the disciplined study of human nature—personology—urgently requires a plan of procedure which is founded upon an adequate conceptual scheme. For research that is unenlightened by basic theory tends to be haphazard, irrelevant, and wasteful. This conviction has led to the proposal of the following tentative hypotheses: (1) The processes which are of final interest to the psychologist are those which organize the experience and control the behavior of living organisms. (2) In higher forms these processes occur in the brain region—at the top of a hierarchical system of dominant nerve centers. (3) It is these organizing and controlling cephalic processes which the psychologist must attempt to conceptualize. (4) The apparently continuous stream of processes is made up of discontinuous, rapidly succeeding occurrences. If this is not strictly correct, it is at least permissible for theoretical purposes to divide the ceaseless flow into very small temporal units—each to be called a *regnancy.* (5) Each regnancy may be regarded as a sensorimotor propagation—a complex integrate of mutually dependent processes which endures for a fraction of a second. (6) According to the double-aspect theory—the most convenient working hypothesis for a psychologist—the

constituents of the regnancy are capable of achieving consciousness. (7) To explain a conscious event as well as to explain a behavioral event, however, one must take account of more variables than are, at the moment, present in consciousness. Therefore, it is necessary to conceptualize unconscious regnant processes. (8) In formulating regnant phenomena, constructs derived from subjective experience are at present more useful than those derived from physiological investigations. It should be understood, however, that every psychological term employed refers to some hypothetical physical variable. (9) A man's life is a long history of successive regnancies, each regnancy being at once a culmination, an assimilation, and a prophecy. (10) A regnancy is an utterly hypothetical, or metaphysical, unit, since it is impossible to catch and inspect a single one of them. (11) A single regnancy is organized and successive regnancies are linked together by the endurance of a particular directional tension—to be called a *need.* (12) A need is a force which (if uninhibited) promotes activity which (if competent) brings about a situation that is opposite (as regards its relevant dynamical properties) to the one which aroused it. (13) Successive stimuli may be bound together by a similarity of signification into a single stimulus-gestalt, to be called a *press.* (14) The endurance of a certain kind of press-perception in combination with a certain kind of need organizes regnancies into a convenient temporal unit, to be called an *event.* (15) A need may be internally evoked in a pressless environment. When this happens, the persistence of the directional tension alone determines the duration of the event. (16) For a personologist an *event* is the most actual "thing" there is, and is the proper matter for analysis. (17) The dynamical structure of an event is given by the particular press-need combination which is exemplified. This is called its *thema.* (18) Endopsychic events and pure phantasies of exopsychic events should be recognized. These also have their *themas.* (19) A thematical (dynamic) analysis is merely one of several different modes of describing an event. (20) An abstract biography, or *psychograph,* may be written in terms of the characteristic *themas* and thematic relations occurring during each stage of a subject's development. (21) A single meeting, or a series of recurrent associations between a subject and a particular object (a thing, an animal, a human, or an institution), is to be

called a *relational*. An enduring relational may be analyzed into its constituent events. The principal relationals may be taken as the basis for a psychograph.

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QUELQUES CONCEPTES FONDAMENTAUX POUR UNE PSYCHOLOGIE DE PERSONNALITÉ

(Résumé)

L'auteur suggère que l'étude systématique de la nature humaine—la personnologie—a grand besoin d'une méthode de procéder laquelle est basée sur un plan conceptuel satisfaisant. Dans ce but, on avance les propositions suivantes: (1) Les processus qui intéressent enfin le psychologue se trouvent dans la région du cerveau. Ce sont ceux-ci qu'on devraient rendre conceptuels. (2) Le cours en apparence continu de ces processus contrôlants est supposément composé d'événements discontinus, se succédant rapidement, dont on peut appeler chacun une régnance. (3) On peut regarder chaque régnance comme une propagation sensori-motrice—une intégralité complexe de processus mutuellement dépendants laquelle dure la fraction d'une seconde. (4) Une seule régnance est organisée et des régnances successives sont liées par la durée d'une tension particulière de direction—qu'on appellera un besoin (impulsion). (5) Un besoin est une force qui (si non inhibée) encourage l'activité qui (si adéquate) fait venir une situation qui est le contraire (à l'égard de ses propriétés dynamiques pertinentes) de celle qui l'a éveillée. (6) Des stimuli successifs liés par la similarité de signification dans une seule stimulus-gestalt peuvent être appelés une presse (presse plurielle). (7) La durée d'une certaine sorte de perception de presses combinée avec une certaine sorte de conation de besoins organise les régnances dans une unité temporelle identifiable, qu'on appellera un événement. (8) La structure dynamique d'un événement est donnée par la combinaison particulière de presse et de besoin laquelle se montre. On appelle ceci son thème. (9) Une biographie abstraite, ou psychographe, d'un individu peut être écrite en termes des thèmes caractéristiques ou des relations thématiques qui ont lieu pendant chaque stage de son développement.

MURRAY

DIE GRUNDBEGRIFFE FÜR EINE PSYCHOLOGIE DER PERSÖNLICHKEIT

(Referat)

Der Autor weist darauf hin, dass das systematische Studium der menschlichen Natur—Persönlichkeit—einen Plan des Verfahrens dringend erfordert, der auf ein hinreichendes Begriffsschema gegründet ist. Diesbezüglich werden die folgenden Sätze vorgeschlagen: (1) Die Prozesse, die von endgültigem Interesse für den Psychologen sind, haben ihren Sitz im Gehirn. Es sind diese, die zu Begriffen gemacht werden sollten. (2) Der scheinbar fortlaufende Strom dieser kontrollierenden Prozesse soll aus unterbrochenen, schnell aufeinanderfolgenden Vorkommnissen bestehen, deren jedes eine Herrschaft genannt werden kann. (3) Jede Herrschaft darf als eine Sinnesmotorverbreitung—ein verwickeltes Ganzes der wechselseitigen, abhängigen Prozesse angesehen werden, die für das Bruchstück einer Sekunde dauert. (4) Ein einzelne Herrschaft wird organisiert und nachfolgende Herrschaften werden durch die Beharrlichkeit einer besonderen Richtungsspannung verbunden, die als Bedürfnis (Trieb) genannt wird. (5) Ein Bedürfnis ist eine Kraft, die (wenn nicht unterdrückt) die Tätigkeit befördert, die (wenn zulänglich) eine Situation

hervorbringt, die derjenigen (in bezug auf ihre passenden dynamischen Eigenschaften) gegenübersteht, die sie erzeugt hat. (6) Nacheinanderfolgende Reize, die durch die Ähnlichkeit der Bedeutung zu einer einzelnen Reizgestalt verbunden wird, darf ein Druck genannt werden (Pluraldruck). (7) Die Beharrlichkeit einer gewissen Art von Druckwahrnehmung in Verbindung mit einer gewissen Art von Bedürfniszusammenhang vereinigt Herrschaften zu einer identifizierbaren Zeiteinheit, die ein Ereignis genannt wird. (8) Die dynamische Struktur eines Ereignisses wird durch die besondere Durckbedürfnisverbindung gegeben, die erläutert wird. Dies wird sein Thema gennant. Eine abstrakte Biographie oder Psychographie eines Individuums darf in bezug auf die charakteristischen Themata oder thematischen Verhältnisse geschrieben werden, die bei jedem Stadium seiner Entwicklung vorkommen.

MURRAY

PERCEPTION AND VISIBILITY THROUGH FOG*[1]

From the Psychological Laboratory of Cornell University

MERRILL F. ROFF

Notwithstanding the fact that the visibility of objects in a foggy atmosphere is of decided importance in aerial navigation, and although it is well known that the state of the atmosphere is an important condition not only for visibility in the narrow sense but also for the perception of the size and distance of objects, little experimental work has been done in this field by psychologists. The present investigation deals with certain aspects of this problem. We employed an artificial fog enclosed in a dark chamber. It is obvious that the optical properties of a fog of this kind, and particularly the action of the fog on visible light of different wave-lengths, must be known before any results concerning the visibility of lights or objects through the fog can be interpreted. Fortunately we have at our disposal considerable literature on energy transmission through artificial fogs, and on both energy transmission and visibility in natural fog. In addition to these studies there are a few observations by psychologists on the perceptual appearance of lights and objects through fog.

Fogs and clouds are essentially similar; they differ only in position. According to Humphreys (10), ". . . fogs consist of water droplets, or ice spicules, condensed from, and floating in, the air." The most frequent diameters of these droplets range from approximately six to 18 microns (9). The effect of a fog on incident radiation may be ascribed to two factors, absorption and scattering. Absorption by the fog-particles themselves is, however, not important, since if all the fog droplets in an artificial fog a few yards long, and dense enough to obscure any incident light, were compressed into one sheet of water, this water would be as transparent as a pane of glass (1). The effect, therefore, must be the result of scattering.

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[1]The writer wishes to express his appreciation of the assistance given him by Dr. H. P. Weld, under whose direction the work was done.

The scattering of light by fog is not a simple reduction of intensity but involves a re-radiation and dispersion of any incident light by the different particles.

There are three main effects of this dispersion of light by fog which are relevant for a perceptual study. (1) Diffuse illumination from a light-source may be transmitted much farther than direct rays from such a light. For instance, a cloudy day may be relatively bright although the sun itself is not visible. (2) Since the light is diffused and does not travel in a straight course from the object to the eye, the background of a light or object is much brighter than it would be with a simple reduction in the intensity of the light. This gives rise to a complex figure-ground situation which is one of the subjects of this study. (3) If the particles in a medium are close to the wave-length of light in size, as is the case in ordinary atmosphere or with smoke-particles, the scattering of the incident light is selective. Light of longer wave-lengths is transmitted directly better than light of shorter wave-lengths. More light of the shorter wave-lengths is directed to one side. It is for this reason that the clear sky appears blue, and that the rising or setting sun appears red. Or, to consider another ordinary situation, the smoke of a cigarette held in the sunlight filters the light so that the smoke appears blue from the side, but the light directly transmitted appears yellowish-brown, if it strikes a white surface. Fog-particles, however, have a diameter from ten to fifty times the wave-length of light and thus do not themselves scatter light selectively, that is, there is no distortion of the color of light passing through.

Benford, Granath and Hulburt, and Breckenridge and Nolan have investigated the transmission of light from different sources through foggy atmosphere. Benford (4), working with a high intensity searchlight, found that, at a distance of 2300 feet in a somewhat water-saturated atmosphere, the relative intensity of the red end of the spectrum increased, while the relative intensity of the blue and violet decreased. Granath and Hulburt (6) measured with a spectrograph the relative percentage of light of different wave-lengths transmitted through natural fog. They found an increasingly better transmission with longer wave-lengths up into the infra-red, but concerning the visible spectrum they conclude that ". . . the present measurements indicate that a red light is practi-

cally no better than a blue for fog penetration purposes." Breckenridge and Nolan (5) compared the relative visibilities of a clear beam of light and of light transmitted through a red filter. They found that at a distance of 2.5 miles the red light is reduced only half as much as the clear light. The results of these investigations agree in demonstrating a slight but constant superiority of transmission with the longer wave-lengths through foggy atmosphere. However, the differences in transmission value between the upper and lower ends of the visible spectrum were in a ratio of only about 1.5 to 1. This tells us little about visibility or perceptibility in foggy atmosphere, since some visual functions (for example, visibility or visual acuity) vary more than this with different colors of equal intensity.

When we turn to the laboratory researches with fog, we find somewhat different results. Utterback, Anderson, and Houghton have studied the relative transmission of light of different wave-lengths through artificially produced fogs, in fog-chambers varying in length from two to six meters. Utterback (17) worked with fogs produced by a rapid expansion of saturated air, and measured the transmitted light with a photometer. He found a maximum transmission in the central part of the visible spectrum; this maximum fell off by 31 per cent at the most poorly transmitted red and by 17.7 per cent at the most poorly transmitted blue. Anderson (1) produced his fog by evaporation and condensation of water around condensation nuclei of (1) tobacco smoke and (2) magnesium chloride mixed with tobacco smoke. The transmitted light was measured by a photoelectric cell. He found transmission minima in the first case at 0.475 microns and in the second case at 0.580 microns, but the differences in the transmission of different colors were slight. Houghton (8) studied the relative transmission of light of different wave-lengths through a fog made by condensing low-pressure steam in a zinc-lined wooden chamber. He measured the transmission with a photoelectric cell. He found a maximum transmission at about 0.490 microns, and, in general, his curves slope downward in each direction from this peak, so that the transmission values of the red and the blue are approximately the same, and differ from that at 0.490 microns by a ratio of about 1.5 to 1.

Perhaps not unexpectedly, since the experimental settings differ

in several respects, the results of these studies do not agree with one another. The resolution of these differences is not our problem. It is enough to note that any transmission differences that appear in these experiments fall within a ratio of 1.5 to 1 between the best and the most poorly transmitted wave-lengths.

A more important consideration is that in all cases the workers with artificial fog report transmission maxima in one part or another of the middle range of the visible spectrum and never in the red part, whereas the investigators with natural fog uniformly report a constantly increasing transmission with increasing wavelength. This is probably due to the presence of dust or smoke particles in natural fog, since there is no reason to believe that the water droplets themselves would affect the light in that manner. This suggests that artificial fog represents in this respect an ideal case, and imposes a necessity for caution in the application of fog-chamber results to natural conditions.

When we turn to the psychological literature, we find that Katz (11), in discussing the modes of appearance of colors, mentions fog as an example of bulky color (*Raumfarbe*). Transparent bulky colors have much the same characteristics as transparent film colors (*durchsichtige Flächenfarben*) except that they appear three-dimensional. Bulkiness (*Raumhaftigkeit*) appears clearly only if the colors are at the same time really transparent; the space, which appears to be filled with color, stands out from its surroundings by virtue of the fact that objects can be seen through it. Katz has observed this phenomenon with natural fogs. He concludes that the clearly three-dimensional character of fog depends on the ability to see and recognize objects through it. If the fog becomes so thick that the perception of objects is definitely impaired, or if one directs one's glance in such a way that no objects are visible, the bulkiness disappears. Under otherwise similar conditions, the thickness of the fog appears to diminish with an increase in the number of objects seen through it.

The term "visibility" has, in many contexts, a specious simplicity, which hides the fact that it is often applied to several different perceptual situations. It is employed in a technical sense as meaning the ratio of the intensity of radiant energy to brightness (brilliance); it is also used to refer to the perceptibility of objects; and there is the

further distinction between the perceptibility of a point of light and the perception of the contours of an object. In a fog, light is diffused, as with a sheet of ground glass; a distinction can therefore be made between the "visibility" or brightness of this undifferentiated field and the more specific "perceptibility" of an object or light source in the field. This distinction can be illustrated by a cloudy day, when the daylight around us may be quite bright, although the sun itself cannot be perceived.

The determination of the relative energy transmission through fog of light of different wave-lengths may yield some information about visibility in this narrow sense, but it does not lead to accurate information concerning perceptibility. Multiplication of the energy values of the different spectral lights by the corresponding values of the human visibility curve produces only an estimate of brightness, and there are more factors involved in most perceptual situations than brightness alone. This fact is often overlooked by persons who apply the values of the "human visibility curve," a curve of the brightness relationships, to visual perceptibility in general.

We have undertaken the investigation of three related problems which emerge from the literature: (1) the visibility and perceptibility of objects through fogs of different densities, (2) the visibility and perceptibility of objects illuminated by light of different wave-lengths, and (3) the phenomenal appearance of the visual fields, particularly (*a*) at the absolute visibility threshold, (*b*) when a definite point of light is just distinguishable from its background, and (*c*) when a visual object is clearly defined.

Apparatus[2]

The artificial fog in all observations reported here was produced by the condensation which followed when low-pressure steam was released into a fog-chamber. The apparatus was located in a dark-room, eight by five meters in size. Steam was obtained from a pressure cooker. A small pipe and petcock were inserted into the lid of the cooker, and to this was attached a length of rubber tubing about

[2]The writer wishes to express his appreciation for the assistance given him by Mr. H. G. Houghton, both in suggestions concerning apparatus and in the interpretation of the results. He is, however, not to be held responsible for any conclusions presented here.

three meters long and one centimeter in internal diameter. The other end of the tubing was inserted into the upper part of the end of the chamber opposite the observer. The fog-chamber was an asbestos-covered tin tube, 20 cm. in diameter and 6 meters in length. It was closed at each end by a tin cap which fitted snugly around the outside of the tube. The inside of the tube and end-caps was painted black to eliminate reflection. Since, however, light was reflected by small water droplets formed on the sides of the tube, two black, sheet-metal discs, with an outer diameter equal to that of the inside of the tube and with circular apertures in their centers with a diameter of 10 cm., were placed vertically in the tube with their plane surfaces at right angles to the line of vision. One was located in the center of the tube, three meters from each end, the other a meter from the end of the tube toward the light-source. These effectively eliminated direct reflections from the light-source, but there still remained slight reflections from the tube, of light diffused by the fog itself. The end-cap or lid at the observer's end contained in its center an opening about three centimeters in diameter, in front of which was an artificial pupil of four millimeters in diameter. Only monocular vision was used. The observer's head was held in place by a chin-rest.

In the center of the tube at the end where the stimulus was located, a small metal block, five centimeters square and three millimeters thick, was imbedded. This served as a framework for a movable disc, in the center of which was an open area in the shape of a broken circle (the International Test Object). The outer diameter of the circle was one inch, and the width of both the circle and the break was 3/16 inch. Thus, the whole circle subtended a visual angle of 11.5 minutes; the break and the ring of the circle subtended an angle of approximately 2.3 minutes. The break in the circle could be changed easily and quickly by hand to any desired direction. No appreciable amount of steam escaped through this circle, and a difficult problem was thus avoided, since it was not necessary to have any glass so close to the fog that it might be clouded by condensation. This circle was used in all the experiments; it gave a clear and simple standard for the determination of what could be seen with different degrees of fog density, although we were not chiefly concerned with the measurement of acuity.

The light-source was placed about six inches behind the object, and was directed on, and in a line with, the object and with the artificial pupil at the other end. The broken circle was the only aperture through which light could enter the fog-chamber. Between presentations the light was obstructed by a simple, hand-operated drop-shutter. All the apparatus except the light-source remained the same during all the observations.

When the tube was to be filled with steam it was desirable that the end of the tube opposite that at which the steam entered be open in order to produce an even distribution of fog throughout the tube. It was therefore necessary to enlist the observers' cooperation in removing the lid from the tube before the steam was released and in replacing it when the steam was shut off. It was found that as much steam as would cause the pressure in the cooker, as indicated by its gauge, to drop from 15 to 10 pounds produced fogs that were similar within the limits discussed below. By the time the cooker was closed, at the proper gauge reading, the steam had begun to flow from the open end of the tube. The steam when first injected remained in the upper part of the tube, but settled into a fog which was more dense 30 seconds after the tube was closed than it was immediately after closing.

One of the factors contributing to the selection of this type of artificial fog for use in this experiment was that Houghton had worked with a similar fog and there were thus available several important facts about the fog itself. According to him (9), the size of the fog-particles is relatively uniform and ranges from two to three microns. Even if larger particles could be produced feasibly, they would settle out more rapidly than the particles used here, and the fog would thus dissipate more quickly. There is no way of determining directly the number of particles per unit volume in the fog; it is sometimes estimated, however, from the size distribution of the particles and the total liquid water content. Since this last factor is very difficult to measure accurately, it has become conventional in laboratory work with fogs to specify the density of the fog by the percentage transmission of a standard light source. Thus, any particular fog density can be specified by the ratio of the intensities of this standard light with and without fog. In our experiments the intensity determinations were made with a Holophane Lightmeter.

It was found in preliminary observations that under our conditions the rate of dissipation of different similarly produced fogs was approximately constant. Consequently, photometric readings were taken of brightness in the fog-chamber at specific times after the tube had been filled and closed. Two different light-sources were used, one a 60-watt frosted Mazda daylight bulb, with an effective intensity of 120 foot-candles, and the other a projection lantern with the lens system at distant focus in order to get parallel rays, as nearly as possible. The latter had a brightness of about 1200 foot-candles at a distance of six meters. When taking the photometric readings the light-sources were projected on the end of the tube, where, instead of the broken circle, a circular aperture was located. The diameter of this aperture was 12 mm. with the projection-lantern, and 30 mm. when the 60-watt bulb was used.

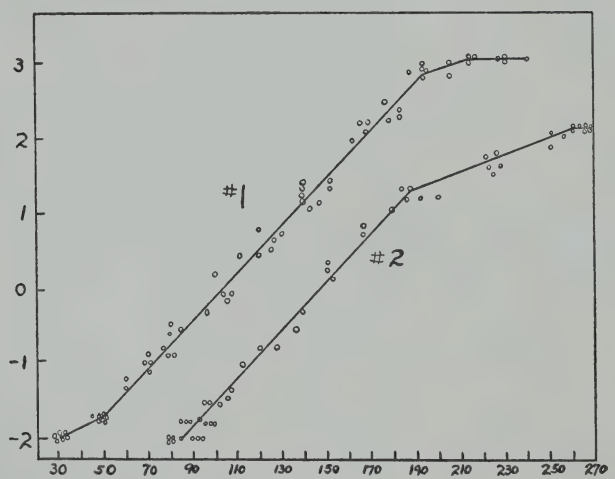

FIGURE 1

The abscissae represent time in seconds after filling of fog-chamber. The ordinates represent log of brightness of field in photometric units. Curve No. 1, projection lantern; Curve No. 2, 60-watt daylight bulb.

The accompanying graphs show the results of these observations. The log of the brightness in foot-candles is plotted against the time after filling the tube. In general, during the interval from 50 to 180 seconds, the brightness-value doubles every ten seconds; during

the interval from 30 to 50 seconds with the brighter light, and from 180 to 240 seconds with the less intense light, the brightness doubles about once in 20 seconds. With the lantern, the fog was less bright at 30 seconds after the tube was filled than immediately after filling, due to the settling of the fog. After three minutes the light became so bright that the photometer settings were difficult to make. With the daylight bulb, light was visible from the beginning, but not until about 80 seconds did it reach a brightness of .01 foot-candles, the lower limit of the lightmeter. In almost no case does the intensity at a given time fall outside the range from one-half to twice the mean value of the intensity for that time; similarly, the time at which a given intensity appears does not vary more than ten seconds on each side of its mean. This indicates that, within these limits of variation, successive fogs were constant; time or intensity differences within this range of error cannot be considered significant.

Experimental Procedure

At the beginning of each observation period,[3] an interval of ten minutes was allowed for dark adaptation. The observers' reports were recorded by a dictaphone. It was found that the time required for these reports, usually about a minute, was so long in relation to the rate of dissipation of the fog, and so variable, that the regularity necessary for quantitative results could not be obtained. The qualitative reports were, therefore, taken first with each experimental setting, and a quantitative series was run separately. In the qualitative series, exposures were made as soon as the report of the previous exposure had been completed; in the quantitative series, exposures were made at regular intervals of either ten or twenty seconds. In the quantitative series, the observers were asked to report at the following appropriate times:

1. The presence of any illumination in the visual field;
2. The presence of the brighter spot which they thought would develop into the object; and
3. The direction of the break in the object if the break was perceptible.

[3]The observers were Dr. A. D. Glanville (*G*), a research fellow in psychology, Mr. J. McV. Hunt (*H*), assistant in the department, and Miss G. L. Bestor (*B*) and Mr. J. D. Coakley (*C*), scholars in the department of psychology.

The direction of the break was reported in terms of time on a clock; it was considered correctly named within an hour on either side of its actual position.

First Experimental Series. The light-source was a 25-watt frosted Mazda bulb inside a light-box. In the end of the light-box toward the object was a daylight filter, through which light was emitted with an intensity of 2.5 foot-candles. This intensity was empirically selected as one which would not penetrate the fog at all until the fog had stabilized considerably, but which was bright enough to cause the object to become clear before the fog had dissipated completely. A small plug, inserted into the circuit, caused the light to flash on and off for 0.8 second. This continuous series could be broken up into pairs of flashes by means of a hand shutter. The light-source was permitted to flash constantly until the light first appeared in the visual field; after that it was exposed in pairs of flashes, at intervals of 20 seconds in the quantitative series, and immediately after the completion of a report in the qualitative observations.

The instructions were:

> Look through the eye-piece at frequent intervals until you can report light. Then observe at the "ready-now" signal, when a pair of flashes will appear. Report briefly concerning your one clearest perception during each pair of flashes. Later, a broken circle will appear; report the direction of the break in terms of position on a clock.

With the first appearance of light, a field was distinguishable only as the merest sort of brightness change, a dark gray barely distinguishable from the darker visual background. The observers were at times unable to tell certainly whether there was something in the field in front of them, or whether it was a subjective phenomenon. Very soon this developed into a filmy field of irregular or indefinite shape, gray, and either poorly localized or appearing very distant. As the fog further dissipated, the field became roughly circular in shape, uniform in brightness, and, although it remained filmy, it took on an appearance of depth.

At the next stage, the field appeared larger and nearer; it was still very filmy, but it became more definitely tri-dimensional; and small portions of the field varied in brightness. The central part of the field became somewhat brighter than the outer portions.

Next, the field became whiter and still larger; it remained filmy and "deep-looking." The "object" first appeared as a spot of white light; it was brighter and more concentrated than its ground. As it brightened it grew and became darker in the center than at its margin. A mist could be seen extending from the object to the observer.

As the object clarified, it became definitely a circle, bluish-white in hue, with filmy edges and a dark center, surrounded by a mass of diffuse light more filmy in character than the circle. Its localization became more definite, both as regards its distance from the observer and its position in the visual field.

TABLE 1

TIMES IN SECONDS AFTER FOG-CHAMBER WAS FILLED AND CLOSED AT WHICH EACH STAGE BEGAN AND THE ELAPSED TIMES BETWEEN STAGES

Observer	First stage	Second stage	Time difference between first & second stages	Third stage	Time difference between second & third stages
B	90	160	70	210	50
C	95	165	70	225	60
G	110	180	70	245	65
H	95	175	80	220	45

Table 1 shows the results of the quantitative observations. Values of the first stage show the time of first appearance of light; values of the second stage are the times when the "object" first appeared; values of the third stage indicate the time of accurate localization of the break in the object. These times are averages for 15 fogs for each observer. In every instance at least two-thirds of the observed values fall within 10 seconds of the average. When the times of the first and second stages are related to the intensity-curve (2) of Figure 1, it can be seen that the visual field in which the object first appears as a definite spot of light (which means the first direct transmission of light from the object to the eye) is about 128 to 256 times brighter than the visual field in which the illumination can just be detected. The exact number here has no particular significance, but it does give a rough estimate of the magnitude of the ratio. In comparing the difference between the second and third stages, it is necessary to select the mean for all observers, instead of computing the time difference for each ob-

server, since the rate of dissipation of the fog changes at 180 seconds. (Before this time the brightness doubles twice as fast as it does later.) If we take 170 as the mean for the appearance of the second stage, and take 55 seconds as the mean time between the second and third stages, it is found that the brightness of the third stage is about ten times that of the second. And finally, if we compute the brightness change between the first and third stages, we get a brightness difference of some 1280-2560 times between the stage when the light is first visible and the stage when the object is sufficiently clear to permit the accurate location of its break.

Second Experimental Series. The light-source was a projection lantern with a 500-watt Mazda bulb, so focused as to emit almost parallel rays. It gave a lightmeter reading of 1200 foot-candles at a distance of six meters. This light was intense enough to penetrate the thickest of our fogs, so that the visual field under these conditions was never without illumination.

The visual field, when the fog had become well distributed, first appeared as a circular disc or cylindrical mass of diffuse, homogeneous, filmy, "penetrable-looking," nearly or completely achromatic light. It was in most cases almost or completely unlocalized. All observers reported slight brightness-differences within the field; each was small in area, shifted constantly, and contributed to an impression of movement. The light had a different hue for each observer, and he was consistent in his reports of color, both at this and at higher intensity levels. *B* called it gray, very slightly yellowish; *C* saw it consistently as bluish-gray; for *G* it was always achromatic; *H* sometimes reported it as yellowish-gray and, at other times, simply as gray.

Later, and along with considerable brightening of the total visual field, the object emerged as a figure. At first, it was merely a brighter speck in the center of the field, which fluctuated into and out of sight. Slightly later, it appeared as a spot of light, yellow and indistinct in shape; it frequently changed both shape and position, so that it was neither at rest in the field nor stable in form. A gray mist also appeared, extending from the object to the observer. With increasing brightness, the object became more definite in form and color. Its apparent position became more stable, both as regards its distance from the observer and its horizontal and vertical

position. It sometimes appeared as being in front of the field around it, while at the same time a haze could be seen in front of the object. The field around the object was a bright yellow; it decreased both in intensity and chroma with an increasing peripheral distance from the object.

Somewhat later the light became glaringly bright. A fog was still apparent between the light and the observer. The background became brighter and less chromatic. Movement in the field was still apparent. Two of the observers discovered independently, and a third, when asked to observe, found that, at a time when the light was so bright that the break in the circle could not be seen at all, it was possible to locate accurately the break by observing the after-image.

In the quantitative series, since light was apparent from the first and the interval between the appearance of the object and the correct localization of the break was short, only the times of accurate location of the break were recorded. These fell generally at about 60 seconds. This indicates a brightness of about .05 effective foot-candles when the circle and break became clear.

Third Experimental Series. Here the apparatus and procedure were the same except that the drop-shutter was changed so that a color filter could be interposed in the light path which formerly had been unobstructed. The filters used were from a set of Wratten monochromatic filters; the ones used chiefly were a red, with a total transmission of about two per cent and the greatest transmission in the spectral range of 0.680-0.700 microns; a green, with a total transmission of about six per cent and a spectral maximum at 0.530-0.540 microns; and a blue, with a total transmission of about two per cent and of greatest transmission in the range of 0.450-0.480 microns. Only one color was shown during any single fog; in six fogs, in which the exposure times were two seconds, the colors were presented in the order: *B, R, G, G, R, B;* and in three fogs in which single exposures of 40 seconds (from 50-90 seconds after the tube was filled) were made, the order was *B, R, G.*

Description of the three colors is best begun with the green; spectrally central to the red and the blue, it exhibited certain peculiarities in common with the red, and certain others in common with the blue. At the time of the first presentations, 30 seconds after the

tube was filled, the appearance of the field was much the same as that described above, without the filter. It was almost completely achromatic, sometimes tinged with green. At 50-60 seconds, the appearance of the field had not changed substantially; it was homogeneous and filmy; it was gray, but a definitely greenish-gray. Then, the central part gradually increased in brightness and hue until finally the small brighter area, which later would develop into the object, emerged. This occurred at about 65-80 seconds.

During the interval at 70-90 seconds this spot brightened and clarified; at the end of this time it was clearly a circle, often with a recognizable break. As it first appeared, it was a small spot or area, indefinite in shape, green, or in some cases yellowish- or whitish-green in color, rapidly increasing in brightness. One observer, *H,* reported that, in the transition from an indeterminate shape to a clear circle, it passed through a brief stage as a double circle; *G,* in a slightly later exposure, "saw the object at first as two interlocking circles, which became one circle." When the spot first appeared, the ground was bright and greenish in the center; it decreased both in hue and intensity toward the edges. As the brightness of the object increased, the outer edges of the central area of the field became relatively brighter. Often the reports at this stage concerning the direction of the break in the circle were inaccurate. As the object continued to brighten and clarify, the break became more accurately distinguishable; later the object became too bright for the break to be visible. At no time was either the circle or the break so clearly defined or sharply contoured as with the red filter at its optimum intensity.

When the red filter was exposed in two-second periods, ten seconds apart, starting at 30 seconds after closing the tube, no light of any character was seen by any observer until 70 seconds, except that once *B* reported "a bare, reddish glimmer" at 50 seconds. At 70 seconds, a reddish light appeared; it was in all cases definitely red as soon as it was seen at all. Aside from its being so definitely chromatic at the first appearance, the field was similar to the other fields in their early stages. However, it changed rapidly. Within ten seconds after the first appearance of light, the object became visible as a circle, and frequently the break was clearly apparent. Within 20 seconds after the first appearance of light, the break in the circle

was usually localized accurately. At this stage the object appeared as a brilliant red circle, sharp of contour, surface-like rather than filmy, with a dark and filmy center. It was the clearest, most distinct circle seen at any time in this experiment. The surrounding ground, within the area of the disc, differed strikingly from the similar grounds with no filter or with the blue filter in that it was completely uniform in hue and brightness from the central object to the edges of the disc. This field was relatively very bright, when compared with similar fields with other filters. In general, the circle was clearer and the ground was brighter than with the other colors at a similar time. Doubleness was never reported at any time with the red color. As the intensity increased, the object increased in brightness more rapidly than the field around it. At a time of four to four and one-half minutes after the start, the break in the circle was still distinguishable.

With the blue filter, light was visible with the first exposure, at 30 seconds, as a round filmy field, nearly or completely achromatic, generally faintly bluish. At 50-60 seconds, the field appeared definitely bluish-gray; the center was somewhat brighter and of better saturation than the outer regions. As this continued to brighten (at about 70 seconds), the object appeared as a small, bright, blue spot. The ground of the object decreased rapidly in hue and brightness with increasing horizontal and vertical distance from the object, and at the edges of the field appeared dark gray or black, sometimes with a tinge of yellow. As the center of this field, including the object, grew brighter, it became more bluish. There remained the decrease in hue and brightness with increasing distance toward the periphery, which was more noticeable in this series than in any other.

As the object brightened and increased in size, it displayed various peculiarities. Its contours were indefinite, its shape irregular, and it moved around in the field, up and down and sideways. It was described as "formless," "unstable," and even as of "indescribable form." With increasing brightness the object entered a stage which lasted until the fog dissipated; instead of appearing as a single circle, it was seen as two or more circles by all observers. These appeared interlocked or superimposed, and the position of the break in the circle could not be seen. A few times the object was seen as single, but in none of these cases could the break be distinguished.

A few presentations were given with a blue-green filter and with a violet filter. Although the apparent hue of the blue-green field was predominantly greenish, the object was seen as double, a characteristic of the blue, but the circles were clearer than with the blue. With the violet the field was visible when first observed, at 30 seconds, although the filter had the lowest transmission value of all. The object, when it appeared, was even more chaotic and unstable than the blue; finally, late in the fog, it clarified into interlocking double circles, but no break was apparent.

Discussion of Results

In the quantitative series with a light-source of 2.5 candles, the difference in brightness of the field in which light was just visible and that in which the object could first be distinguished from the surrounding ground was found to be about 128-256 times; that is, this was the difference between the just visible, diffusely illuminated field and the field in which light was first directly transmitted from the object to the eye. Not until this second stage was any definite localization of the light-source possible. The size of this difference as found in our experiment is much less than that occurring under certain conditions of daylight. Barrows (2) reports that a noon-time sun in September produces a ground brightness of about 8200 foot-candles with a cloudless sky; but with the sky so densely clouded that the position of the sun could not be seen, a September noon-time sun produces a ground brightness of about 1600 foot-candles, about one-fifth of the intensity with a cloudless sky. His data do not show the brightness when the sun becomes just visible as an object, but the value must lie somewhere above 1600 foot-candles. However, even if we regard 1600 foot-candles as the measure of this intensity, the ratio between this value and that for the least visible brightness is enormous.

These differences are of obvious importance for any use of the term "visibility." Where "visibility" is strictly defined, as it seldom is, it has a specific and technical meaning. In the report of the Committee on Nomenclature and Standards of the Illuminating Engineering Society, the following definition is given (16). "Visibility of radiation of a particular wave-length is the ratio of the luminous flux at that wave-length to the corresponding radiant flux."

Troland (16) says that a visibility curve is "a plot of the reciprocal of the radiant intensity required, at various wave-lengths, to evoke equal brilliance." Visibility so defined is, in the peculiarly diffuse field created by a fog, essentially unrelated to the perception of specific objects. Any incident light is so diffused, and the relation between the brightness of a field and the perceptibility of figures in it is so indefinite, that a consideration of visibility, in this sense, is almost irrelevant to the problem of the "visibility of objects." This disparity between visibility and object-perception may be less noticeable under clear atmospheric conditions, but with fog it becomes prominent.

With the colored light-sources the outstanding phenomena were (*a*) the late appearance of illumination with the red filter, (*b*) the differences in clearness of the object with different colors, (*c*) the doubling of the object with the filters of shorter wave-length, and (*d*) the differences in character of the areas surrounding the object.

With a red light-source in the spectral range of 0.680-0.700 microns and a transmission factor of about two per cent, no light was visible at all until the physical intensity was about eight times the intensity when the light from the blue source was easily visible. Since Houghton (8) has shown that there is no such difference in energy transmission between red and the other colors in this type of fog, this lag in time of appearance is obviously attributable to the eye; it seems clearly a case of the Purkinje phenomenon. In scotopic vision the absolute visibility of red is much the poorest of all the colors. When illumination from the red source does appear, it is seen as red from the first; green and blue on the other hand appeared nearly achromatic at first. This is referable to the known absence of the photochromatic interval with red of this wave-length. After the red hue appears the contours of the red object become distinct more rapidly than do those with the other colors, although the green object, which had an initial superiority in radiant flux, could be seen distinctly at about the same stage. The blue object never became satisfactorily distinct.

Although we have accepted Houghton's finding that the relative transmission value through fog of different wave-lengths is practically the same, and have referred the seen differences to the physiology of the eye, it should not be overlooked that in the above dis-

cussion we are dealing with diffuse light, and not with directly transmitted rays on a background of diffused light. The photo-cell, by means of which the physical energy is measured, cannot distinguish the qualities of the energies received, i.e., it cannot distinguish figure and ground. Consequently, for psychological purposes, Houghton's findings may be misleading if an inference should be made concerning the effect of the energy transmission of the different colors perceived. For example, in one case the figure might be contributing 50 per cent of the energy and the ground 50 per cent; whereas, in another the figure might be furnishing 75 per cent and the ground 25 per cent; an equal energy transmission would then produce differently organized visual fields. This is of particular importance with figures of different color.

There were marked differences in the pattern of the visual field when the light-source was varied in color; these can best be distinguished in those fields in which the circular object was at its clearest for that color. With a red filter, the circle and its break were clearer and more distinct, while the ground was vivid and uniform in hue and brightness. The green circle was somewhat more diffuse, less clear cut, and its break was less easily apparent; its ground at the lower brightness-levels was similar in organization to that of the blue; at higher intensities, like that of the red. The blue circle was decidedly less distinct than the other two; it often appeared double, and in some fogs the break in the circle was not accurately located at any time. Its ground was bright immediately around the circle, but shaded off rapidly in brightness and saturation toward the periphery of the field; in the dark edges of the field, tinges of yellow were sometimes seen.

When we turn to a consideration of already known facts of color perception, we find that several of these differences are referable to ordinary human vision in a perfectly clear atmosphere. Helmholtz (7), in his discussion of chromatic aberration, points out that "In an ordinary rectangular spectrum projected on a white wall some distance away, . . . while the red end may appear fairly well in its proper form, the violet end is more or less blurred." A relevant investigation in this region is that of Belaeiv-Exemplarsky (3), who found that red and yellow appeared more compact or concentrated, *"fester," "stärker zusammengeknüpft,"* more material, and possessing

a more clear-cut object character than green or blue. Green and blue, the short-wave side of the spectrum, were *"flüssiger,"* airier, and more immaterial; they were blurred (*verschwommen*) in appearance. Concreteness (*Körperlichkeit*) in colors brings with it the impression of definite localization; such colors seem to be marked off from their surroundings. Red had sharp boundaries, while blue had indefinite ones. All these phenomena appeared here; whether or not the fog exaggerated them is in most cases difficult to decide. It is certain, however, that the fog did not eliminate them; obviously, then, any discussion of perceptibility as opposed to mere visibility must take these facts into account.

Concerning clearness of detail with objects of different colors, it has been shown by Pauli (14), who investigated visual acuity with different photometrically equal colors, that there are significant differences. He found the following values, expressed as ratios of the different acuities:

Red is to White as 1 is to 1.4
Green is to Red as 1 is to 3.3
Blue is to Green as 1 is to 2.7
Blue is to Red as 1 is to 8.6

Although we did not take a sufficient number of observations with colors to yield significant results, a consideration of the results we have suggests that they might well approach those of Pauli if taken in sufficient number. Aside from this, Pauli's results have interest as demonstrating clearly the disparity, with different colors, between the "visibility" of the human visibility curve, and visual acuity, which would be included under our "perceptibility."

Luckiesh and Moss (13), using a filter which eliminated the blue rays from a light-source originally covering the whole spectrum, found that, although the intensity of illumination was somewhat reduced, the visual acuity was about the same or better than the acuity without the filter. They remark that "We have reason to believe that a corresponding gain would not result if the red end were shortened instead of the blue end." Here again is an illustration of a disparity between visibility and perceptibility.

Let us consider one more set of results, this time concerning the relation between figure and ground. Langmuir and Westendorp

(12) found that with a point source of light in a field which was varied in brightness the threshold visibility of a red source was relatively independent of the background illumination, that a green light was affected in a manner similar to a white one, and that the threshold of a violet light varied much more than that of any of the others with a change of brightness of its ground. The same effect was found in our observations; the red object seemed to act more independently of its ground than did the other colors.

Although the superiority for object-perception of the red over the blue seems generally established, nowhere in the literature have we found mention of a doubling of an object of short-wave illumination with monocular vision. The regular relationship of this doubling with wave-length of the lights makes it seem certain that the phenomenon is not an artifact of the filters. It may be peculiar to the present set of fogs, because the size of our fog-particles is less than that of most natural fogs.

A consideration of the appearance of objects of different color leads us again to emphasize the fact that measurements of transmitted energy can be brought into calculable relation with human vision in terms of brightness alone, and brightness or visibility is only one of many perceptual factors. We may conclude that, since the physical studies at least agree on the statement that the transmitted energy differences between different parts of the spectrum are small, consideration of the perceptibility of objects in fog must be closely related to the empirical situation in which the perception is to occur.

Summary

An apparatus for the production of artificial fogs is described. A review of the physical literature showed that there has not yet been developed a generally accepted physical theory of the transmission of light through fog, and that, because of certain differences in the action of natural and artificial fogs on visible light, results obtained with artificial fogs can be extended to cover natural fog only with caution. It was found necessary to distinguish between *visibility,* as meaning the relationship of brightness to energy, and *perceptibility,* in connection with the perception of objects.

1. A phenomenal description is given of the visual fields in arti-

ficial fogs with different sources of illumination and at various levels of brightness.

2. With a light-source of 2.5 foot-candles intensity, the difference in brightness between the visual field in which the presence of light can just be detected and the field in which an object can just be distinguished is in a ratio of 1 to 128-256 times. The difference in brightness between the latter field and one in which accurate report could be made of detail in the object is in a ratio of about 1 to 10.

3. With light-sources of different color, obvious figure-ground differences appeared; consequently, knowledge of relative energy transmission will not yield accurate information concerning perceptibility.

4. If visibility be used as referring to the first appearance of brightness in the visual field, red is the poorest of the colors; if, however, some specific detail of an object is to be seen, red as a source of illumination is relatively better than the other colors used, and better, in proportion to its intensity, than a nearly white light; its "perceptibility" is better.

5. With a blue light-source the object, when visible, generally appeared double and was never at any time as clear as the other objects. This doubling did not occur with the red. Green light, in general, produced phenomena intermediate to those of red and blue.

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LA PERCEPTION ET LA VISIBILITÉ DANS LE BROUILLARD

(Résumé)

On décrit un appareil pour la production des brouillards artificiels. On donne une description phénoménale des champs visuels dans les brouillards artificiels avec diverses sources d'illumination et à divers niveaux de clarté. Avec une source de lumière de 2,5 bougies-pied d'intensité, la différence de clarté entre le champ visuel où l'on peut découvrir justement la présence de lumière et le champ où l'on peut distinguer justement un objet est dans une proportion d'environ un à deux cent. La différence de clarté entre le dernier champ et celui où l'on a pu faire un rapport exact des détails dans l'objet est dans une proportion d'environ un à dix. Avec des sources de lumière de couleur différente, des différences manifestes entre la figure et le fond se sont montrées. Si l'on se sert de la visibilité à l'égard de la première apparence de clarté dans le champ visuel, le rouge est la pire des couleurs; si cependant quelque détail spécifique d'un objet est à voir, le rouge comme source d'illumination est relativement meilleur que les autres couleurs employées, et meilleur, en proportion de son intensité, qu'une lumière presque blanche. Avec une source de lumière bleue l'objet, quand visible, s'est montré généralement double, et n'a jamais été à aucun temps aussi clair que les autres objets. Ce doublement ne s'est pas montré avec le rouge. La lumière verte a produit en général des phénomènes intermédiats à ceux du rouge et du bleu.

ROFF

DIE WAHRNEHMUNG UND SICHTBARKEIT IM NEBEL

(Referat)

Ein Apparat zur Erzeugung künstlicher Nebel wird beschrieben. Eine phänomenale Beschreibung der Gesichtsfelder in künstlichen Nebeln mit verschiedenen Quellen der Beleuchtung und bei verschiedenen Graden der Helligkeit wird gegeben. Bei einer Lichtquelle von 2,5 Fusskerzen der Intensität steht der Unterschied der Helligkeit zwischen dem Gesichtsfeld, in dem das Vorhandensein des Lichtes eben entdeckt werden kann, und dem Feld, in dem ein Gegenstand eben unterschieden werden kann, in dem Verhältnis von ungefähr eins zu zwei hundert. Der Unterschied an Helligkeit zwischen dem letzterem Feld und einem, in dem ein genauer Bericht über Einzelheiten im Gegenstand gemacht werden konnte, ist im Verhältnis von ungefähr eins zu zehn. Bei Lichtquellen verschiedener Farben erschienen deutliche Figur-Grundunterschiede. Wenn die Sichtbarkeit in bezug auf die erste Erscheinung der Helligkeit im Gesichtsfeld gebraucht wird, so ist Rot die schlechtste der Farben; wenn aber irgendeine bestimmte Einzelheit eines Gegenstandes gesehen wird, so ist das Rot als Quelle der Beleuchtung verhältnismässig besser als die anderen gebrauchten Farben, auch besser in Verhältnis zu seiner Intensität als ein beinahe weisses Licht. Bei einer blauen Lichtquelle erschien der Gegenstand, wenn sichtbar, gewöhnlich doppelt und er war niemals so klar wie die anderen Gegenstände. Diese Verdopplung kam beim Roten nicht vor. Grünes Licht ergab gewöhnlich Phänomene zwischen denjenigen des Roten und Blauen.

ROFF

OCULAR BEHAVIOR AND THE PRINCIPLE OF PICTORIAL BALANCE*[1]

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THE PROBLEM

The significance of the sequence of ocular fixations made while viewing a picture is frequently commented on by those trained in art and in the literature on composition and design. Henry R. Poore (16), author of three books on pictorial composition, one of which is published in its 14th edition, associate of the National Academy of Design, winner of several prizes and medals for painting writes:

> . . . *it is important that the eye have such a course marked out for it,* lest, left to itself, it slip away through the sides, and the continuous chain of reverie be broken.

And,

> . . . the various items of a composition are taken at their required value *to the extent to which they adhere to and partake of the established plan of observation.*[2]

The above quotations emphasize the order in which the eye fixates the various parts of a picture as a factor in the appreciation of a pictorial composition. Correlative to this doctrine is the one that the artist can plan the visual sequence by which the observer views the picture. In the same book, Poore (16) adds: "The artist may make use of this fact in balancing the picture and of *leading the eye out where he will.*"[3] This quotation leaves no doubt of the

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[1]This is a condensed report of a dissertation accepted for the Ph.D. degree by Stanford University in 1934. The research was financed by the Endowment for Psychological and Psychical Research established by Thomas Welton Stanford. The author is indebted to Dr. Walter R. Miles, Dr. Paul R. Farnsworth, and Dr. C. P. Stone for suggestions pertaining to this investigation.

[2]Italics by original author.

[3]Italics by present author.

author's belief that the artist can control the movements of the eye made while surveying a picture.

This evaluation of eye-movements must of necessity have been established by the method of introspection, or at least without the aid of refined instrumentation. Yet, research by Stratton (20) and others into the character of eye-movements of reading and of regarding curved lines has shown that introspection gives an erroneous report. These facts lead one to question the reliability of introspective observations of ocular behavior in looking at pictures.

Unreliability of introspection of eye-movements and the assumption of the importance of movements of eyes in the appreciation of pictorial composition point to a gap in the field of experimental aesthetics which can be bridged by experimentation. Since an investigation of the eye-movements and their relations to the appreciation of pictorial composition is too large for this study, this research is restricted to ocular behavior and the *principle of pictorial balance*. Balance was chosen because it is elementary and offers the possibility of quantitative expression of results.

Obviously the "balance" of a picture exists only in the judgment of the observer; this is true, even for a symmetrical design. Therefore, this study must deal with the *judgment* of the *balance* of pictures and eye-movements made while viewing them. The hypothesis to be investigated is—*there is a relation between the eye-movements made while looking at a picture and the judgment of the balance of the picture.*

This hypothesis is not original with the writer, but is held by certain authorities in the field of art. Puffer (17), Langfeld (7), and Angier (1) seek to explain the judgment of the balance of a visual composition from the balance of motor impulses, and all express the opinion that, even though these motor impulses may be distributed throughout the body, the balance of movements of the eyes elicited by the composition may be the basis upon which the judgment is made. Henry R. Poore (16) in at least three places in one text states that there is a causal relationship between eye-movements made in observing pictures and the judgments of their balance. Goldstein and Goldstein (3), in a widely used book in applied design, remark:

> In working for a balanced room, one should continually test both halves to see that one half does not present any greater attraction to the eye than the other.

And again,

> While the objects on each side of the fireplace are not alike, the eye is held for the same length of time by each group, and a balance results.

So much for the belief by some authorities in a relation between the pattern of ocular fixations and the judgment of the balance of pictorial compositions.

Two possible objective tests of this hypothesis occur to mind. The most apparent is that of *comparing the judgment of the balance of the picture with the eye-movements made during the observation in which the judgment of the balance of the picture is formed.* The other test is that of *comparing the judgment of the balance of the picture with the eye-movements made during the initial viewing of the picture with no mental set other than that of enjoying* it. Differently put, what is to be the mental set of the observers at the time the eye-movements are recorded? Are the observers to have the mental set of judging the balance of the composition, or are they to be observing the picture for the purpose of enjoyment?

Stratton (21) found that the pattern of eye-movements elicited by a symmetrical design was more nearly symmetrical when the subject was in doubt as to the symmetry of the figure than when enjoyment was the motive for observation. He concludes that our enjoyment of symmetry cannot be attributed to symmetry of eye-movements evoked by symmetrical design. These results strongly suggest that, if eye-movements were photographed during the judgment of the balance of the pictures, a symmetrical pattern of fixations would be found, irrespective of the judgment of the balance of the picture. Moreover, the point of view of those holding to the hypothesis under discussion is that the movements of the eyes along the course which is "marked out" for them by the picture result in pleasure or dissatisfaction. Discontent may lead to analysis of the difficulty, which may be unbalance. If the course "marked out" for the eyes does not include equal attraction for them on both sides of the composition, a judgment of unbalance will result. Puffer (17), Angier (1), Langfeld (7), and the Goldsteins (3) subscribe to this explanation. From the point of view of aesthetics a larger contribution can be made by searching for a relation between free eye-movements of the initial viewing of a composition and the judgment

of its balance than by seeking a relation in eye-movements made while *forming* the judgment of the balance of the composition.

The following outlines the plan by which this study sought a relationship between the eye-movements made in the initial and free observation of pictures and the judgment of their balance.

1. Judgments of 40 subjects on the balance or lack of balance of each of 51 pictures were obtained.
2. The three pictures most consistently judged to be balanced, the three most consistently judged to be overbalanced to the right, and the three judged most consistently to be overbalanced to the left were selected for use during photography of eye-movements.
3. Photographic records were made of the eye-movements of each of 30 to 40 subjects observing these nine critical pictures. Subjects had not seen the pictures before recording and were *uninformed* that the research dealt with pictorial balance. The subjects were asked to look at the pictures as if they were on the wall of a living room.
4. Eye-movement records of the subjects who made free observation of these critical pictures were compared with the judgments of their balance by the original group of raters.
5. Ratings of the balance of the nine pictures by the eye-movement subjects were secured in the same situation in which the eye-movement records were made.
6. These ratings were compared with those made by the group which rated the pictures for balance. The purpose of this comparison was to justify the assumption that the balance of the pictures would be judged the same in the record-taking situation as in the first rating situation.

Subjects were all young men, ages 16 to 30 years, most of whom were undergraduates at Stanford University. The limitation of sex avoided the problem of sex difference. No person was used who had more than five quarter hours (or the equivalent) of training in pictorial art. Art training was presumed to include courses in Design, Pictorial Composition, Drawing, Painting, Art Appreciation, and History of Art. Untrained subjects were used because this research is in the field of the consumption of art, and the majority of art consumers are not formally trained; and an untrained group probably is less heterogeneous in its knowledge of and attitude toward pictures than a trained group.

Balance

An evaluation of the principle of balance as not merely elementary but essential appears in the literature on design and composition. H. R. Poore (15) writes, "Of all pictorial principles none compares in importance with Unity or Balance." And, "The test to which the sensitive eye subjects every picture from whatsoever creed or camp it comes is *balance* or equipoise, . . ." Thus Eugen Neuhaus (13) expresses the same idea. "To the well-trained artist who comprehends the rules this is so easily achieved that examples of bad composition in the matter of balance are relatively rare." Allen Tucker (24) appraises the principle with the statement: "Balance is of the very greatest importance. If any object does not balance no peace can be had with it. No matter how untutored one may be, any object badly out of balance produces a great feeling of discomfort." And in the same article, "Picture balance is a necessity; . . ."

Although no very specific rules can be given for the attainment of this quality in a design, because each design or picture presents unique problems and opportunities for its attainment, judgment of balance is rather definite. Meier (10) found that high school students untrained in pictorial art recognized balanced elements in a composition.

Mental states which form the basis of such judgments have received the attention of writers in the field of fine art and in philosophical and experimental aesthetics. Descriptions of these states can be found in the writing of Neuhaus (13), Jacobs (5), and Langfeld (7). Attempts to express the idea of pictorial balance frequently include the use of an analogy to mechanical balance. Such references are used by Tucker (24), Poore (16), Puffer (17), and Gordon (4).

There must be a balance not only about the vertical axis but about the horizontal axis as well. Although there is disagreement in the literature as to the relative importance of these two aspects of balance, such a question is pedantic from the artistic point of view because a successful picture must be balanced about both axes of its composition. Yet, for research purposes it is well to study the various aspects of balance separately. Hence two problems emerge—balance about the vertical axis and balance about the horizontal axis.

We deal with the former. It is the opinion of the writer that the layman in art is more responsive to this kind of balance, therefore it is more easily understood by untrained subjects. The writer also believes that an experimental investigation of a single aspect of balance may be made without destroying the aesthetic character of the stimulus objects.

Experimental Studies of Balance in Visual Art

Most of the available information on the principle of pictorial balance is the result of uncontrolled introspections of artists, art critics, or aestheticians. Experimental studies of this topic are limited in number. Puffer has made the only two which bear any considerable relation to this study. Other investigations, although studies of some of the factors of balance in a picture, treat these factors as isolated elements. Such studies are of limited value because these factors were isolated from their aesthetic situations. Experiments on color as a factor of balance have been made by Pierce (14), Warden and Flynn (25), and Koch (6). Results of these researches do not agree. R. P. Angier (1) studied lines of unequal division which, nevertheless, gave the impression of balance. He accounted for these results with the hypothesis that the center of interest is at the point of division and that eye-movements or even innervations involving the whole motor system are "equal and similar" in both directions from the point of intersection. Yet his report of this investigation includes no experimental evidence for his hypothesis that eye-movements are a factor in the judgment of balance. One hundred and thirty Barnard College students were given the opportunity by Lund and Anastasi (9) to improve asymmetrical or unbalanced geometrical designs by adding lines to them. Those additions were made which resulted in figures which were either symmetrical or informally balanced. In one of Puffer's (17) several investigations of balance, subjects were confronted with an oblong piece of board covered with black cloth on which was placed a strip of white cardboard 1 centimeter by 8 centimeters; they were requested to adjust another strip of twice the length so as to produce the most pleasing effect. She reported that when the strips of cardboard were placed upright, the relative positions of the strips corresponded to the principle of mechanical balance.

If the strips were placed at an angle, and those slanting away from the center were considered as "heavy" and those slanting toward the center as "light," the same principle was found to apply. Her other studies (17), employing the same technique, showed that depth will serve to balance a near object placed farther from the center, and likewise an interesting object will serve to balance an uninteresting one placed at a greater distance from the center. She concludes that size or mass, depth in the third dimension, direction, and interest are pictorial elements which are to be reckoned with in obtaining picture balance. The factor of direction was defined as "direction of *motion or attention* (of persons or objects in the picture), an ideal element, that is; and direction of line." Following these investigations, black and white reproductions of 1000 good pictures were analyzed. These pictures were classified into religious, portrait, genre, and landscape groups, and generalizations were offered as to the characteristic use made of the above-mentioned factors of balance in each of these classes of pictures.

Summarizing the available information on the principle of pictorial balance:

1. Most of the literature is an expression of opinion.
2. There are few experimental data upon which to base conclusions.
3. With two exceptions, experiments which have been reported deal with situations which are not pictorial.
4. The hypothesis proposed by Angier, Puffer, Langfeld, Poore, and the Goldsteins, that there is a relation between ocular behavior and the judgment of balance of the composition, has not been examined by any reliable objective method.

The purpose of this research was to study the judgment of the balance of pictures and its relation to eye-movements made in the undirected viewing of them by the use of an objective technique.

Selection and Preparation of Pictures for Rating

Controls reside in the selection of pictures to be observed rather than in the choice of subjects. Because of the importance of the pictures the standards employed in their selection need consideration. Those chosen satisfied the following seven criteria:

1. *Pictures Must Be Not Well Known.* Judgments were to be

made upon initial viewing of the pictures. This criterion was apparently successfully met inasmuch as only one of the pictures was recognized and that by only one subject. It is probable that this unfamiliarity secured a higher degree of attention than would have been devoted to well-known examples.

2. *Pictures Must Contain No Conspicuous Figures or Faces.* Portraits or figure studies might attract attention almost exclusively to a few restricted areas—a situation undesirable in the research. Because of these limitations, choices were all landscapes or pictures of still life.

3. *The Technique Employed in the Production of the Pictures Must Be Excellent and Conservative.* Distractions from the composition, whether due to poor drawing or other defective technique, were not permissible. In addition, distorted drawing and other forms of craftsmanship which are acceptable in art, but which do not present objects in common visual form, were avoided.

4. *Approximately Two-Thirds of the Pictures Should Provoke Judgments of "Unbalanced."* The need for the judgment of some of the pictures as "balanced" and others as "unbalanced" has been set forth. Yet a search for pictures of excellent but conservative technique convinced the experimenter that few of them would be judged as "unbalanced" by the subjects to be used. Artists who have achieved excellent technique seldom err in this elementary principle. Consequently, the opportunity for a judgment of "unbalanced" was provided by trimming one or more of the edges of certain pictures. The success of this "unbalancing" technique is manifest in the results.

5. *Trimming Must Not Be Evident in Pictures So Treated.* Trimmed pictures must be "unbalanced" but not reveal the trimming.

6. *Pictures Should Contain No Small but Important Details.* This requirement was made in deference to the technique of recording eye-movements.

7. *Pictures Should Have No Blemishes.* A picture defective because of poor photography or printing or because of flaws in the paper would not elicit normal responses either of judgments of balance or of ocular behavior.

Fifty-one pictures which satisfied these criteria were offered to the subjects for rating in the form of enlarged photographic reproduc-

tions of oil paintings, water color paintings, and prints.[4] Photographs were made from reproductions which appeared in such art periodicals as the *American Magazine of Art, The Arts,* and *The International Studio,* and from books. Enlargements were made on paper having no gloss (matte) and a rough surface because it preserves the visibility of the picture by avoiding glare. None of the pictures used was colored. It must be admitted that color may be an important factor in the balance of a picture, but a picture rendered in only black and white can be judged for balance; therefore, the omission of color does not vitiate the research—it merely limits the problem in a rather important respect. The largest picture presented to the subjects was 26.0 x 33.7 cm.; the smallest, 18.4 x 24.1 cm. Their shapes varied from a vertical rectangle, the ratio of the sides of which equalled 1.00 to 1.51, to a horizontal rectangle whose two sides were in the same proportion.

Each of these photographs was mounted in a mat made of commercial mat-board which, although it bears the trade name of black, is a dark gray. The extensions of the mats beyond the horizontal pictures were: top—8.9 cm., bottom—11.4 cm., sides—10.2 cm.; for vertical pictures: top—7.6 cm., bottom—12.7 cm., sides—8.9 cm. There were slight exceptions in the case of two pictures. These matted pictures were mounted on Sunset Board, a heavy backing which gave strength necessary to withstand handling.

Rating Pictures for Balance: Procedure

Ratings were made in an improvised dark-room measuring 3.58 by 4.08 meters. There were in the room a chair in which the subject sat, two tables against opposite walls, an easel on which to display pictures, and a curtain of heavy black cloth hung behind the easel to absorb surplus light. The distance of the rater's eyes from the picture was approximately 2.75 meters. It varied from time to time as the rater changed his position in the chair, the location of which was fixed.

So important is the easel that it should be described in detail. The shelf accommodating the pictures was so placed that its upper

[4]Photographic reproductions of these pictures as presented to the subjects are to be found in copies of the thesis in the library of Stanford University.

surface was invisible to the subjects. The front edge was beveled from the bottom so that it and the under side of the shelf were invisible in deep shadow. By the use of a spirit level this shelf was adjusted to a horizontal position. Pictures were placed in the center of the shelf and against a background of cardboard 58 cm. square covered with natural color monk's cloth. Behind this light background there was a black cardboard screen, which concealed the upper part of the easel and absorbed light.

The light for the pictures was approximately 45 cm. from their centers. It cast a fairly even light over them and produced no shadows on their surfaces. A Mazda 60-watt, 120-volt, blue bulb with inside frosting was used. This was the only source of illumination during rating. The shade confined direct light to the picture and its immediate vicinity so the remainder of the room was dimly lighted by reflection.

Before rating the pictures, the subjects were asked to fill out an information blank containing the following items:

Name
Date
Age
Classification—Freshman, Sophomore, Junior, Senior, Graduate Student.
College courses taken in Design, Pictorial Composition, Drawing, Painting, Art Appreciation or History of Art.
Quarter Units (Semester unit equals 1.5 Quarter Units).
Interest in pictures: None, slight interest, moderate interest, considerable interest, great interest.
Approximately how many times a year do you visit art galleries?
Do you engage in any creative pictorial effort such as drawing, painting, photography or modeling? What?
Give the name of the painters of any good original pictures or reproductions of them which you have in your home.
What art magazines do you read or look through?

They were then given the following four tests:

Visual acuity—Lowell's Test Type (Modification of Snellen's Test Type).
Test for astigmatism—chart on Snellen's Test Letters.
Test for handedness.
Test for ocular dominance.

Tests for visual acuity and astigmatism were given only for the purpose of detecting and eliminating subjects whose sensory defects precluded adequate vision of pictures. A paper-cutting test was used to determine handedness. Although ocular dominance is not known to be a factor either in one's judgment of the balance of pictures or in the ocular behavior of looking at them, it was deemed best to hold this factor constant. Inasmuch as right-eye dominance is approximately twice as frequent as dominance of the left eye, those having the former were employed. The test used for dominance is the A.B.C. Vision Test devised by Dr. Walter R. Miles (12). Those showing left ocular dominance also rated the pictures for balance, enabling a comparison with the ratings by those testing dominant in the right eye.

Having completed these tests, the subjects read the following typewritten instructions for the judgment of balance of pictures:

> You are asked to judge the Balance of the composition of a number of pictures. The following offers an explanation of the meaning of the term *BALANCE*.
>
> Imagine the cloth-covered board which you see on the easel in front of you to be a picture, the pencil in the center to be its only support, and that the picture is free to revolve about this axis. A balanced picture gives the feeling that under these conditions it would hang level; one which is overbalanced to the right gives the feeling that under these conditions the right side would sag, and conversely one overbalanced to the left would sag to the left. The estimated weight of the objects depicted is not the sole criterion of balance. For example, a range of mountains on one side of the picture might be satisfactorily balanced by a small tree on the other side. Not just the materials portrayed but the impression of the entire picture is to be considered. Make your judgments by the way you *feel* about the BALANCE of the picture, using any criteria which seem pertinent.
>
> The pictures are to be rated on a scale of five steps. *Please use all five.*
>
> Balanced—to be reported as "balanced."
>
> Slightly overbalanced to the left—to be reported as "overbalanced to the left, Class 1."
>
> Definitely overbalanced to the left—to be reported as "overbalanced to the left, Class 2."

Slightly overbalanced to the right—to be reported as "overbalanced to the right, Class 1."
Definitely overbalanced to the right—to be reported as "overbalanced to the right, Class 2."
None of the pictures to be shown is extremely out of balance so do not hesitate to judge Class 2 of overbalance to the right or left. There is no time limit.

Then raters were orally tested for mastery of the classification. They were assured that this was in no wise a test of their aptitude in pictorial art. Each picture was to be rated on its own merit, irrespective of those previously rated. Emphasis was placed upon careful rating regardless of the time required. It was also explained that, only as a guide in future research, the experimenter would take a record of the amount of time used in judging each picture. No pictures were used as examples.

Pictures were stacked in two equal piles, one on each of the tables, against opposite walls. With the subject in place the experimenter placed a picture on the easel in such a manner that it was invisible to the subject until the experimenter said "Ready" and stepped aside. Taking a position in the dark at the side of the room, back to the rater, he awaited oral expression of the judgment. The decision announced, the picture was stacked on the table, face to the wall. In this manner the subject was denied direct comparisons with pictures already rated. He was informed of a plan of stacking the photographs according to ratings and asked to check the accuracy of placement of pictures he had judged. Time elapsing between the exposure of the picture and the announcement of judgment was recorded. Pictures were presented in a chance order determined by shuffling playing cards. The effect upon judgments of placing and removing pictures from a single side of the easel was compensated by presenting and removing half the pictures from the right and half from the left. Alternate sides were used initially for successive subjects. Thirty-five to 60 minutes were required for the preliminary tests, instructions, and ratings. Although a few of the subjects complained that 51 pictures were too many, most of them appeared interested in the task throughout the series. Records of judgments were made on a check sheet from the stacks of pictures after the subject left.

Results of Judgments of Balance of the Pictures

The critical pictures sought as a result of these judgments were three pictures most equably judged to be "balanced," three most consistently judged to have the greatest overbalance to the left, and three most consistently judged to have the greatest overbalance to the right. Statistical selection of the nine critical pictures demanded numerical weights for the five classes of judgment. The following weights were assigned:

overbalanced to the left—Class 2	1
overbalanced to the left—Class 1	2
balanced	3
overbalanced to the right—Class 1	4
overbalanced to the right—Class 2	5

A mean of 3.00 indicates an average judgment of "balanced" by the group which the mean represents, the one having the smallest mean was judged to be most overbalanced to the left, and the one having the largest mean most overbalanced to the right. Dispersion was expressed by the standard deviation of the distribution. Those pictures were selected for the study of eye-movements whose means were of the appropriate magnitude and whose sigmas of distribution were lowest. Table 1 gives the means and standard deviations of the distributions of judgments for the 51 pictures. The number of subjects who rated each picture is 40, all of whom tested right-eye-dominant.

These data confirm the statement by Meier (10) that untrained subjects can deal successfully with the problem of pictorial balance. It is significant that the standard deviations of the judgments of the balance of 72 per cent of the pictures is less than one rating step. This indicates that for most of these pictures there is a close agreement among judgments.

Critical pictures are starred in Table 1. The three pictures having the lowest means, that is, those which were judged by the group to be most overbalanced to the left, are numbers 1, 2, and 3, having means of 1.750, 1.875, and 1.925. Their sigmas of the distributions are .733, .842, and 1.058. The relatively large dispersion of judgments for number 3 dictated its replacement by picture number 4, having a mean of 1.950 and sigma of .733. The five pictures

TABLE 1

MEANS AND STANDARD DEVIATIONS OF DISTRIBUTIONS OF THE JUDGMENTS OF THE BALANCE OF 51 PICTURES BY 40 UNTRAINED SUBJECTS

Number of picture	Mean of distribution of judgments	Standard deviation of distribution of judgments	Number of picture	Mean of distribution of judgments	Standard deviation of distribution of judgments
*1	1.750	.733	26	3.000	.922
*2	1.875	.842	**27	3.075	.755
*3	1.925	1.058	28	3.100	1.091
4	1.950	.733	29	3.150	.726
5	2.075	1.149	30	3.150	.726
6	2.100	1.300	31	3.150	1.130
7	2.250	1.410	32	3.325	.848
8	2.275	.774	33	3.325	1.034
9	2.325	1.330	34	3.475	.836
10	2.325	.905	35	3.500	.806
11	2.500	1.049	36	3.650	.760
12	2.550	.545	37	3.675	.818
13	2.550	.835	38	3.675	1.104
14	2.550	.740	39	3.725	.836
15	2.575	.863	40	3.725	1.162
16	2.575	.833	41	3.750	.915
17	2.600	.700	42	3.775	.961
18	2.625	1.391	43	3.775	.851
19	2.675	1.058	44	3.975	.758
20	2.675	.685	45	4.000	.707
21	2.850	.792	46	4.100	.831
22	2.900	.889	47	4.150	.989
**23	2.975	.851	48	4.175	.891
**24	2.975	.651	***49	4.200	.678
25	3.000	1.072	***50	4.275	.744
			***51	4.500	.837

*Critical pictures used in eye-movement recording to represent the class of pictures judged to be "overbalanced to the left."

**Critical pictures used in eye-movement recording to represent the class of pictures judged to be "balanced."

***Critical pictures used in eye-movement recording to represent the class of pictures judged to be "overbalanced to the right."

whose means most nearly approach 3.00, judged to be most nearly balanced, are numbers 23, 24, 25, 26, and 27; having means of 2.975, 2.975, 3.000, 3.000, and 3.075, respectively. Because of the smaller dispersion of their distributions, pictures number 23, 24, and

27 were selected. Although none of the three has means exactly at 3.00, all differences are well within the standard error of the mean, the value of which is approximately .110. The three pictures having mean judgments indicating greatest overbalance to the right are numbers 49, 50, and 51, having means of 4.200, 4.275, and 4.500. Their sigmas of distribution are .678, .744, and .837.

Are the differences between means for pictures of separate groups large enough to be significant? Critical ratios computed for the determination of significance of the differences between the least diverse means in separate groups are both over 7.00. Although the critical ratios of the separation of the most divergent means within single classes are 1.76 or less, they are not complete evidence of homogeneity of the groups, because the ratios might become significantly large if a greater number of judges were used.

Incidentally a comparison of the results of the judgments of 40 right-eye-dominant subjects and 22 having left ocular dominance yielded a correlation between the means of the judgments of the two groups for each of the 51 pictures of $+.94 \pm .105$, and a critical ratio of the difference between the means of the mean judgments of the two groups of .370. Both values are eloquent of agreement in the judgments of the balance by raters differing in ocular dominance.

Technique of Recording Eye-Movements

Dodge (2) has set forth the requirements of the ideal technique of recording ocular behavior and defends the method of corneal reflection as satisfying them. Excellent reviews of the literature on the development of techniques of recording eye-movements are to be found in articles by Tinker (22) and Schmidt (19).

The question of the accuracy of the fixations demands attention. Perhaps ocular fixations are made on a large area and a position of the eyeball which includes any part of this area suffices visually. If so, the method of corneal reflection would give an error in recording fixations. However, Weymouth (27) and others found that the central axis of the eyeball and the line of regard approximate each other within 22′ or less. This fact, coupled with proof by Dodge that the corneal reflection is a reliable index of the position of the eyeball, leads to confidence in this method of recording ocular behavior.

This study requires both horizontal and vertical records of eye-movements. Weaver (26) has devised the economical and simple method used for making simultaneous horizontal and vertical records from one beam of light. A pair of glass prisms of acute angle placed in the region of the camera lens deflected one-half of the cone of light coming from the lens to the right and the other half to the left. These two spots were brought to a focus on separate films, one moving vertically, the other horizontally.

The present set-up used a single prism. This arrangement produced two images—one deflected and one in line. To avoid a distortion of the record due to an unequal displacement of the image by the prism, the undeflected beam exposed the horizontal record, and the deflected image recorded the vertical movements.

Eye-Movement Recording Apparatus

Arc Lamp and Housing. The source of light for the reflected beams was an E. Leitz-Wetzlar automatic-feed carbon lamp placed 90 cm. from the subject's eye, and in such a position to his right that the reflection appeared in the middle third of the cornea of the right eye during ocular movements required in the research. A box contained the lamp and confined the light. The beam passed through a cobalt blue glass, which reduced its visual intensity without impairing photographic registration, and through an opening 3 cm. in diameter in a cardboard screen to the subject's eye.

An adjustable head-rest prevented the subject from moving his head, because such movements would invalidate the records. A biting-stick held firmly between the front teeth aided subjects to avoid head movements. A photograph of this device with a subject in place appears in Figure 1. These devices preserved immobility for the short time necessary for records for each picture.

Screen for Controlling the Subject's Vision. A screen made of dark gray cardboard, large enough to include all but the extreme periphery of the subject's field of vision, was located 24 cm. in front of the subject's eyes. An aperture at the right admitted the beam to the subject's right eye, the lower one allowed this beam to be reflected from the cornea into the camera lens, and the large rectangular one restricted binocular vision to the picture displayed on the easel.

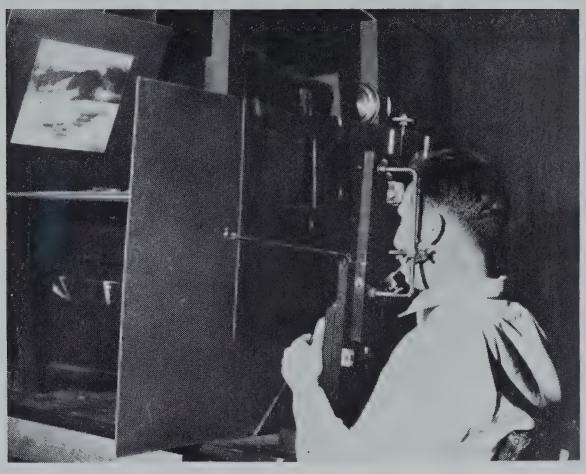

FIGURE 1
SUBJECT IN POSITION FOR RECORDING EYE-MOVEMENTS
(EXPLANATION IN TEXT)

The time unit employed was one twenty-fifth of a second. A disc turning one revolution per second, in which 25 notches had been cut, supplied these units. To facilitate reading records for time values, every fifth partition between the notches was omitted, forming a differentiation of every fourth unit. This interrupting disc was located in the path of the beam between the camera lens and the screen mentioned above. Such a position avoided distracting the subject by the flickering of the beam, which occurred when the disc was placed between the lamp and the subject. The rotating disc was mounted directly on a Telechron Synchronous Motor which proved by repeated tests to run at a uniform rate.

Immediately in front of the camera lens was a flag-shaped metal shutter which controlled accurately the amount of time for making records for each picture.

The Bausch and Lomb Protar lens of the camera was approximately 32 cm. from the subject's eye. At 178 cm. from the film, it gave five times magnification. The prism for deflecting half the beam of light was mounted directly behind it.

Mechanism for Moving Film.[5] Vertically and horizontally moving films were synchronized by film sprockets geared to the same electric phonograph motor. Brass guides were installed at appropriate places in the light-tight exposure box to prevent the film's moving transversely and so invalidating records. Both Du Pont and Eastman 35-mm. panchromatic films were used.

Exhibition of Pictures. The pictures observed for record were located above the camera barrel on an easel invisible to the subject. (See Figure 1.) An inconspicuous device held the photograph tilted forward in a plane perpendicular to the line of regard. A vertical screen made of black cardboard immediately behind the picture, between it and the easel, obscured the easel and other parts of the apparatus. With his head in position the subject was able to see only the screen immediately in front of him, the picture, and a small part of the screen back of the picture. Because records were required of the ocular behavior the first time the subject saw the picture, the manner of exposure was important. This was done quickly and without interference by a falling cardboard screen. This black screen, 48 cm. square, hinged to the top of the camera barrel, fell forward at the tripping of the mechanical release exposing the picture. A platform, installed appropriately to establish a pneumatic cushion between it and the falling screen, caused the latter to alight silently and without jar. The opening of the flag shutter was synchronized with descent of the falling screen, so that the film record began the moment the picture came to view.

The distance of the center of the pictures from the observer's eye was approximately 91.0 cm. Angular movement required of the eyeball for viewing the extremities of the largest dimension of any picture was 19° 24′ and the greatest for the extremities of the smallest was 11° 34′.

Illumination of Pictures. The distance of the lamp from the centers of the pictures was 50 cm. A 30.0-cm. tubular, 40-watt, 120-volt Mazda bulb was enclosed in a cylindrical shade 7.5 cm. in diameter and 31 cm. long having an opening of approximately 45 degrees along its length and an inner surface covered with aluminum paint.

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[5]The writer is indebted to Dr. Homer Weaver who designed the gear box which with some modifications was used for driving the film.

Controls for the apparatus were at the rear of the camera near the film exposure box. By this arrangement the experimenter was completely obscured from the subject during recording.

It has been assumed that ocular behavior in this experimental situation is the same as that under normal conditions. Present methods of recording eye-movements permit no comparison of usual ocular behavior of picture viewing with that at the camera. Although the subjects stated that neither the arc light nor the head-rest affected the manner of viewing, such evidence is of slight value because of the unreliability of introspective report of ocular behavior. Robinson (18) has attempted to determine the validity of the eye-movements made under the conditions necessary for photography. Comprehension and rate of reading during recording were compared with the same in reading under normal circumstances, and no significant differences were found. He concludes that ". . . measurements made during photography may be considered to be valid measures of normal reading." The present writer does not argue by analogy that, since *reading* has been shown to be as *efficient* under experimental as under normal conditions, the eye-movements of looking at pictures would, likewise, show no significant difference. Yet it does suggest that, if such a comparison could be made, no considerable differences would appear.

Procedure of Record-Taking

Preliminary to taking records, the subject was given the same tests taken by those who rated the pictures. All the tests of vision were given without glasses because records were to be made without them. No one was used who showed any noticeable degree of astigmatism, whose distance vision tested poorer than 6-10, or who was left-eye-dominant according to the Miles A.B.C. Vision Test. Then the subject filled out a blank disclosing his training and interest in art. Only the untrained were employed. He was told the purpose of the research was to photograph the movements of the eye made while an observer looked at certain pictures for the first time. Care was taken to avoid the establishment of any mental set other than that of looking at a picture. He was asked to view the pictures "just as if they were on the wall of a living room." After arranging the height of the chair to suit his stature and the demands

of the camera, the head-rest and biting-stick were adjusted so the head was kept firmly but comfortably immobile. Then the camera was focused on the beam reflected from his eye. There followed a trial run of record-taking employing an inconsequential picture to acquaint him with the procedure. Previous to the exposure of the picture, the subject looked through the rectangular aperture at the "falling screen." The experimenter, stationed at the back of the camera, checked the focus, started the film, gave the warning signal, "Ready," released the "falling screen" which exposed the photograph, and simultaneously opened the flag-shutter permitting the beam to strike the film. After the required time had elapsed, the flag-shutter was momentarily closed to produce a break in the record marking the end of the picture record. The shutter was then reopened and the subject was asked at command to look at the four corners of the picture in a prescribed sequence. This produced the reference record employed for locating fixations in the manner discussed later. The subject was allowed to move about the room before making another record. Time of recording for each picture was arbitrarily set at the period which would include the time employed by 95 per cent of the raters for balance in making their judgments of that picture. For pictures 1, 2, 4, 23, 24, 27, 49, 50, and 51 records ran 23, 21, 20, 28, 23, 21, 28, 23, and 30 seconds respectively. A chance order of presentation of the pictures was used and was varied for each subject. *At the close of the record-taking,* each subject rated the nine pictures for balance according to instructions for rating given above. Conditions for these ratings were like those for recording, except that the "falling screen" was not used for exposure and the film-driving mechanism was still.

Technique of Reading Eye-Movement Records

Since fixations must be determined in both the horizontal and vertical dimensions of the picture, any economical system should permit simultaneous reading of horizontal and vertical records. However, fixations cannot be determined accurately by direct use of a projection lantern or common pantograph, because the eyeball changes its position less in making a vertical excursion of a line than in making a horizontal movement over a line of the same length. Miles and Shen (11) have established this fact. The present writer found

considerable individual differences in this ratio. To enable reading the two records concurrently, and to compensate for individual differences in the horizontal-vertical ratio, the writer devised a system of levers which is readily adjustable to the disparity in magnitude of vertical and horizontal records. This mechanism, referred to as a differential pantograph, is depicted in Figure 2. The adjustment

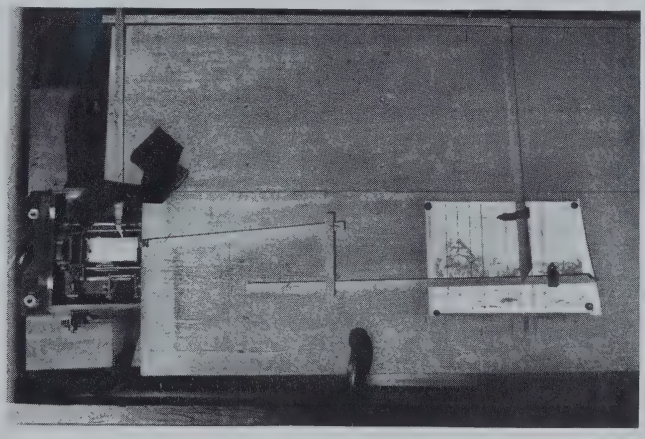

FIGURE 2
DIFFERENTIAL PANTOGRAPH
(EXPLANATION IN TEXT)

of the levers to the requirements of particular records was readily made by placement of the pins which served as fulcrums for the wooden arms of the system. This simple means afforded tight, smoothly functioning bearings.

The device was operated in the following manner. Looking through the circular aperture, the observer adjusted the points of the levers to coincide with the film records. These record-points, although not coinciding at the place of intersection of the two records, were made to refer to its vertical and horizontal axes. Accurate placement of the levers was obtained by manipulation of their picture-ends, the movements of which were 16 to 20 times as great as the record-points. During these adjustments, the observer kept his eye at the aperture, thus reducing the subjective factor in establish-

ing the location of the fixation. Figure 2 shows how the determination of the placement of the fixation on a blue-print record of the picture was made by following to their intersection the horizontal and vertical axes determined by the picture-ends of the levers. At the beginning of the plotting of each blue print, the fulcrums of the levers were adjusted so that the points on the picture-ends of the levers indicated the corners of the picture in agreement with the film record made for this purpose when the subject observed the corners at command.

The accuracy of the differential pantograph was found by two independent readings of the same record. A rectangular field, approximately the size of the average of the nine pictures, having 25 dots evenly distributed over it, was used as a test object. Data comparing the first and second readings are expressed in angular movements of the eye. The range of differences for 31 fixations was 8′ to 42′, with a mean of 20′ ± 2′ and a $\sigma_{dis.} = 10'$.

Although no absolute check on the accuracy of the entire system of taking and reading records can be made, the following evidence suggests the reliability of the complete technique. A record was made of the ocular fixations by a trained subject on the previously mentioned rectangular pattern of dots at command, so at a designated time the subject looked at a given dot. The film record so made was read by reference to the original pattern of dots. The differences between the locations of the dots and the determination of the locations of the fixations by the method are expressed in angular deviations of the eye in Table 2.

TABLE 2

RELIABILITY OF METHOD OF RECORDING EYE-MOVEMENTS. DIFFERENCE BETWEEN POSITION OF DOTS AND RECORDS OF FIXATING DOTS BY A TRAINED SUBJECT

Number of fixations	26
Range of differences, 15′–1°	23′
Mean of differences	42′
Standard deviation of distribution	18′

Since the largest errors appear at the corners of the field, and during picture viewing relatively few fixations were made at these points, the reader may be assured that the average error of locating fixations in the research proper was smaller than that expressed above. Fur-

thermore, the subject may not have fixated exactly on the spots, in which case the errors expressed are too large. And if so, part of the error indicated in Table 2 is not the fault of the method. Finally, the errors are biased in no manner which would affect the results of this study.

Tinker (23) has established the fact that head movements can be accurately discerned in the eye-movement record. Those records disclosing head movements were discarded.

Results of Eye-Movement Recording

Statistical analysis of these data takes the form of a comparison of the ocular behavior of viewing the right and left halves of the pictures. Fixations and the amount of time spent in pause durations were reduced to percentages of the total number of fixations or of the total pause durations made in regarding the picture. Such a translation was demanded because the amount of time and the number of fixations for a given picture differed for each subject. Differences in the number of fixations are due to varied length of pause durations among subjects. Differences in the sum of pause durations are due to the varying amounts of time spent looking off the picture, and to the inequalities in the total time devoted to change of fixation. Fixations made off the picture were not considered.

Six comparisons were used for each of the nine pictures. All include ocular behavior of the entire group in looking at the right and left halves of the specimen. The following comparisons have been made:

1. The percentage of the total time of regarding the picture devoted to pause duration on the left half with the percentage of time devoted to the right.
2. The percentage of the total number of fixations made on the left half with the percentage of fixations on the right.
3. The percentage values for the pause durations were weighted by their distances from the central vertical axis of the picture. The sums of these weighted values for the right and left sides were compared.
4. The percentage values for the fixations were weighted by their distances from the central vertical axis of the picture. The sums of these weighted values for the right and left sides were compared.

5. The mean position of the time spent on the right half with that for the left. (This measure was obtained by dividing the numerical values described in No. 3 by the total percentages of time spent in pause durations for the respective halves.)

6. The mean position of the fixations made on the right with those made on the left. (This measure was obtained by dividing the values described in No. 4 by the total percentages of fixations for the respective halves.)

These six variables were computed for each picture viewed by a subject. All measures of the same kind for one side of a given picture were classified, and compared with the distribution of the same measures for the opposite side. Critical ratios, involving the correlation of the variables, were employed for ascertaining the importance of the differences of the means of these distributions. They are presented in Table 3. Of the 54 critical ratios only three indicate

TABLE 3

CRITICAL RATIOS COMPARING OCULAR BEHAVIOR OF LOOKING AT RIGHT AND LEFT HALVES OF NINE CRITICAL PICTURES. OBSERVATIONS MADE BY UNTRAINED SUBJECTS

T = percentage of pause duration
d = deviation from central axis
F = percentage of fixations
L = difference in favor of the left half of picture
R = difference in favor of the right half of picture

Picture	T	F	Txd	Fxd	$\frac{Td}{T}$	$\frac{Fd}{F}$
No. 1	1.312 L	.899 L	.817 L	.963 L	1.037 L	.538 L
No. 2	2.337 L	2.547 L	2.129 L	1.988 L	.440 L	.314 L
No. 4	.884 L	1.161 L	.993 L	1.473 L	1.286 L	1.768 L
No. 23	2.351 L	2.443 L	2.053 L	1.946 L	.121 R	.000*
No. 24	2.063 L	2.066 L	1.610 L	1.813 L	1.063 R	.788 R
No. 27	.880 L	.803 L	.319 L	.145 L	2.423 R	1.774 R
No. 49	.619 L	1.053 L	.799 R	.426 R	4.300 R	3.739 R
No. 50	.687 R	.411 R	1.037 R	.565 R	.921 R	.895 R
No. 51	2.425 R	2.855 R	2.611 R	3.293 R	2.041 R	2.957 R

*Means equal.

significant differences. They are: (1) the mean position of the time of pause duration for picture number 49; and (2) the mean position of the fixations for picture number 49; and (3) the weighted fixations for number 51. The critical ratios for the fixations for picture number 51 and for the mean position of fixations for the same

picture are large enough to indicate that the probability of accounting for the differences by chance is very small.

If there were an observable relationship between the judgment of balance of pictures and the measures of ocular behavior employed, the critical ratios for the pictures judged overbalanced to the left should be significantly large and in a constant direction; the same should obtain for the group of pictures judged overbalanced to the right except that the direction of difference should be opposite; pictures judged to be balanced should, however, give critical ratios which are not large enough to indicate reliable differences in the ocular behavior for the right and left halves of the pictures. The data obtained do not fulfill these conditions. Table 3 discloses that none of the critical ratios for the group judged overbalanced to the left is large enough to be significant, though all are in favor of the left sides of the pictures; that 16 of the 18 critical ratios for the group of pictures judged overbalanced to the right favor the right sides, three of the 16 are significant, and the remaining two favor the left sides; for the "balanced" class, no ratios are significant, yet these ratios cover a range to the right and left almost as great as the combined range of the other two groups. If the number of subjects were increased, some of the critical ratios might increase enough to indicate significant differences. Inspection of critical ratios reveals a number of the ratios for the *balanced* group which are so large that they threaten to become significant with an increase of N. Therefore the use of a greater number of subjects offers a large probability of the same conclusions as those drawn from the present data. Hence the conclusion that none of the six measures used for comparison differentiates or promises to differentiate any of the three classes of pictures whose balance is variously judged.

If it is true that a composition which holds the eye for the same length of time on each side of the center is judged as balanced, these records should disclose that fact. In 63 of the observations of pictures, at least 45 per cent of the total time was devoted to each side of the picture. To support this assumption the subjects should have given ratings of "balanced" in these 63 cases, yet only 30 per cent of the judgments were "balanced," 27 per cent "overbalanced to the right," and 43 per cent "overbalanced to the left." No evidence for the above assumption appears in these percentages. It should be re-

membered that the judgments were made by the subject *after* the eye-movement records were taken and in the same situation.

Since the statistical comparisons show no positive results, several qualitative comparisons which might show relationships have been made. If the declarations of the authors of texts on pictorial composition are correct, one would expect the construction of the composition to provoke the same pattern and sequence of ocular fixations from all observers. Such is not the case. Comparison by inspection offers striking proof that both the sequence of fixations and pattern of fixations differ markedly from subject to subject. Notwithstanding the fact that the patterns of fixations vary widely for different subjects, perhaps a given part of the composition attracts the first fixations. Individual records show the first three to five fixations made by a subject in surveying a picture located in the same region; but this region of initial viewing varies and may be found at almost any part of the rectangle.

Possibly some distinctive characteristic exists in the pattern of fixations made by the group of subjects in looking at each of the pictures. To enable such an observation, composites were prepared composed of fixations made by the entire group of subjects as they looked at a given picture. The data presented above show that the method of recording is accurate enough to indicate a pattern clearly if such were present. No such distinctive character appears in the pattern of the fixations for separate pictures or for pictures of a given class. The distributions of fixations seem to bear slight relation to the configuration of the picture. Individual records have been inspected for characteristic patterns of fixations resembling the composition. As in the case of the composites, resemblances were inconsiderable.

Fundamental to this research is the assumption that the nine critical pictures would receive the same judgments of balance in the eye-movement recording situation as in the original rating. Although these two situations varied in several respects, the author is of the opinion that the most important difference was that the nine critical pictures were part of a series of 51 for the first group of raters, whereas the eye-movement subjects rated only the critical pictures. A judgment of these critical pictures according to the same rating scale of five steps might be expected to result in differences of judgment due to the absence of less distinctive examples to serve as foils.

Yet, for pictures judged unbalanced, there should be no reversal of direction. That differences in illumination may be a factor in producing differing judgments has been shown recently by Wilcox and Morrison (28). Since the level of illumination at which lighting becomes a factor was far below the degree of illumination used in each of these rating situations, variations in judgments are not attributable to this cause. Table 4 contains the critical ratios of the differences between the means of the judgments of the original group of raters and those of the group of eye-movement subjects.

TABLE 4

COMPARISON OF RATINGS OF THE BALANCE OF NINE CRITICAL PICTURES BY TWO GROUPS OF RIGHT-EYE-DOMINANT RATERS. CIRCUMSTANCES OF RATING WERE DISSIMILAR FOR THE GROUPS

Code number of picture	Means of rater's judgments	Means of judgements of eye-movement subjects	Diff. of means / σ Diff. of means
1	1.750	1.387	2.318
2	1.875	2.333	2.759
4	1.950	2.091	.865
23	2.975	2.935	.222
24	2.975	2.667	1.584
27	3.075	2.786	1.818
49	4.200	3.500	3.848
50	4.275	4.281	.037
51	4.500	4.444	.311

N of the first group was 40, in the second group it varied from 28 to 33. The table shows that with the exception of picture No. 49 there are no significant differences between the judgments of the two groups of raters. Yet, all of these differences might become significant if the *N* were sufficiently increased. The only reversal of direction occurred for picture No. 27. Since this picture is judged to be balanced, reversal is no surprise, especially with a difference inconsiderably small. More important is the question of the uniqueness with which the pictures are judged. The difference between classes of pictures is significant in all cases except No. 49. In this case the mean and that of the picture in the balanced class having the mean nearest it yield a critical ratio of 2.97, which so closely approximates the commonly accepted index of significance as to cause little concern. These facts indicate that, for the purposes of this

research, the two groups of raters made essentially the same judgments of the balance of the critical pictures.

Summary and Conclusions

The purpose of this study is to investigate the assumption by such authorities as Angier, Langfeld, Puffer, Poore, the Goldsteins, *et al.*, that there is a relation between the judgment of the balance of a picture and the behavior of the eyes observing it. Their assumption is that a relation exists between the ocular behavior of the *undirected* viewing of the picture and the judgment of its balance. This investigation was limited to the relation between the judgment of balance about the vertical axis and the eye-movements made in the initial and free viewing of the picture.

Judgment of the balance of 51 photographic reproductions of paintings and prints was obtained. To provoke judgments of "unbalanced," some of the compositions were partly but not obviously obscured. Sixty-two Stanford men students, untrained in pictorial art, and whose ocular dominance had been determined by the Miles A.B.C. Vision Test, were used as subjects. The judgments of the balance of these pictures by 40 men who tested right-eye-dominant were the basis for the selection of nine critical pictures for use as samples to be observed during photography of eye-movements. As a result of these judgments, three groups of pictures were chosen—three judged to be balanced, three judged to be most overbalanced to the right, and three judged to be most overbalanced to the left. A statistical comparison of the means of the judgments of the pictures in different groups shows the classes to be significantly distinct; but no reliable differences appear between means within a group.

The 37 subjects used for making records of ocular behavior while looking at the nine critical pictures were also Stanford men students untrained in pictorial art. Pictures were shown to them first during recording. They were asked to look at the picture "as if it were on the wall of a living room." No mention was made of the principle of pictorial balance before recording. Duration of photographic record varied from 20 to 30 seconds for different pictures. Judgments of balance were made by 95 per cent of the raters in less time than the duration of the record. After making records for the nine pictures, the subjects rated them for balance in the

record-making situation. A comparison of the means of the judgments of this group of subjects with those of the right-eye-dominant raters showed that the two groups gave essentially the same ratings to the pictures. The Miles-Weaver modification of the Dodge method of corneal reflection was used for making simultaneous vertical and horizontal records of movements of the eyes. Film records were transposed to blue-print reproductions of the pictures by means of a modified pantograph, which is adjustable to the differences in individual records and to the varying ratio of vertical to horizontal records.

The ocular behavior of the group in looking at the right and left halves of each of the nine pictures was statistically compared by use of six quantitative measures, all suggested by theories of the relation of the judgment of balance to ocular behavior. Comparisons, necessarily qualitative, were also made and conclusions drawn therefrom.

Within the limits of this research the data support the following conclusions regarding the judgment of the balance of pictures, the ocular behavior of viewing pictures, and the judgment of the balance of pictures and its relation to the ocular behavior of the initial and undirected viewing of them by young men untrained in art.

1. Untrained subjects agree in the judgment of the balance of most pictures.

2. Subjects testing right-eye-dominant and those testing left-eye-dominant by the Miles A.B.C. Vision Test agree in their ratings of pictures for balance.

3. The ratings of the balance of nine pictures were shown to be essentially the same in two different situations.

4. Patterns of ocular fixations made by different individuals looking at the same picture show wide differences.

5. Patterns of fixations made by the group of subjects viewing each of the pictures disclose no unique characteristics for pictures judged to be balanced or unbalanced.

6. Initial ocular fixations on a composition vary in location with subjects.

7. Subjects devoting equal pause duration to each side of the central vertical axes of pictures do not consistently judge those pictures as balanced when rating them in the same conditions in which the eye-movement records were made.

8. None of six measures used for comparison of ocular behavior on the right and left halves of the pictures shows consistent reliable differences for any of the pictures judged to be balanced, overbalanced to the right or to the left.

9. This investigation reveals no relation between the judgment of the balance of a picture and the duration, position, number, or pattern of ocular fixations on either side of the central vertical axis during free observation of the picture.

Consideration of Conclusions

It may be objected that, if descriptions of the ocular behavior of observing pictures and its relation to the principle of balance are erroneous, they would have gained but little acceptance. Stratton (20) has shown that the only method of observation of ocular behavior which is available to most authorities on art, namely that of introspection, is unreliable. This explains the inaccuracies of observations by artists and critics and also the uncritical acceptance of these reports by others.

A possibility of accounting for the negative results which cannot be conclusively refuted by this research is that the records of eye-movements may have been too brief. This criticism can be extended to absurdity, for regardless of the length of record a critic might insist that a longer record bears the possibility of positive results. The fact that records ran longer than 95 per cent of raters required to form judgments of balance is partial refutation of this objection.

Perhaps the negative results are due to examples of unbalance that were too slightly out of balance. But it has been shown that these pictures were judged by the group of 40 subjects to be out of balance by more than twice the amount necessary to establish the unbalanced groups as distinct from the balanced groups.

The writer suggests that the adherents of the hypothesis under question are actually referring to the sequence and the duration of the fixations, not of eye-movements, but of *attention*. Hence the negative results herein reported. Yet, the use of this term is to be found in only a few of the discussions. Liddel (8) has shown that fluctuations of attention and shifts of ocular fixations do not necessarily occur at the same time.

Apparently the function of the movement of the eyes in the appreciation of pictures is to provide views of the parts of the picture which are organized into a whole centrally.

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LE COMPORTEMENT OCULAIRE ET LE PRINCIPE DU BALANCEMENT PICTURAL

(Résumé)

L'hypothèse qu'il existe une relation entre l'évaluation du balancement pictural et le comportement oculaire causé par la composition a été étudiée selon des méthodes objectives. Cinquante-et-une peintures ont été évaluées pour le balancement pictural par quarante sujets mâles non entraînés. Les neuf "peintures critiques" choisies selon les évaluations résultantes ont été trois évaluées le plus balancées à gauche, trois évaluées le plus balancées à droite, et trois évaluées balancées. On a montré statistiquement que ces classes ont été disparates d'une façon signifiante. Pendant qu'un second groupe de 37 sujets mâles non entraînés ont vu les "peintures critiques" pour la première fois, on a fait des enregistrements photographiques des mouvements de leurs yeux. L'observation de la moitié droite et de la gauche des peintures a été analysée dans le nombre et la position des fixations oculaires et la position des durées des pauses oculaires. Ces deux sortes de données ont été comparées chacune statistiquement pour les deux moitiés de chacune des neuf peintures. Trois différentes méthodes d'évaluation ont été employées pour les fixations et les durées des pauses. Les résultats amènent à la conclusion qu'aucune de ces six caractéristiques du comportement oculaire ne fait une distinction précise entre les trois classes de peintures considérées. D'ailleurs, la forme de la fixation ne montre aucun trait unique pour les peintures d'une classe donnée de balancement.

LANGFORD

DAS AUGENVERHALTEN UND DAS PRINZIP DES BILDERGLEICHGEWICHTS

(Referat)

Die Hypothese, dass es ein Verhältnis zwischen der Beurteilung des Bildergleichgewichts und dem Augenverhalten, das durch die Zusammensetzung erzeugt wurde, gäbe, wurde vermittelst objektiver Methoden untersucht. Einundfünfzig Bilder wurden in bezug auf Bildergleichgewicht von vierzig ungeübten männlichen Versuchspersonen beurteilt. Die neun "kritischen Bilder," die nach den gegebenen Urteilen ausgewählt wurden, waren folgende. Drei, die mit dem Übergewicht zur Linken beurteilt wurden, drei mit dem Übergewicht zur Linken beurteilt wurden, drei mit dem Übergewicht zur Rechten, und drei mit einem Gleichgewicht. Dass diese drei Klassen bedeutsam verschieden waren, wurde statistisch nachgewiesen. Während eine zweite Gruppe von 37 ungeübten männlichen Vpn. ihren ersten Anblick der "kritischen Bilder" erhielten, wurden photographische Aufnahmen der Augenbewegungen gemacht. Die rechten und linken Hälften der Bilder wurden in die Anzahl und Stellung der Augenpausendauern eingeteilt. Diese zwei Arten von Daten wurden in bezug auf die zwei Hälften von jedem der neun Bilder statistisch verglichen. Drei verschiedene Gewichtsschemas für Fixierungen und Pausendauern wurden gebraucht. Die Ergebnisse führen zum Schluss, dass keins dieser sechs Merkmale des Augenverhaltens zwischen den drei Klassen der Bilder zuverlässig unterschied. Weiter, das Gebilde der Fixierung weist keine einmalige Eigenschaft für Bilder einer gegebenen Klasse des Gleichgewichts auf.

LANGFORD

THE EXPERIMENTAL CONTROL OF THE HUNGER-DRIVE*

W. A. Bousfield and M. H. Elliott

There are two basic difficulties in the way of quantitative manipulation of motivation. The control of motivated behavior, involving as it does a complex organic alteration, is necessarily rather indirect. Furthermore, it is sometimes evident that a number of distinct mechanisms are involved. The purpose of the present paper is to examine these two difficulties as they are concerned in the case of the hunger-drive. The treatment of other basic drives would involve greater difficulty because less evidence is available, but it is a reasonable assumption that certain general principles would be applicable in all cases.

Methods of Controlling the Hunger-Drive

The large number of methods and practices which have been used in controlling the hunger motive makes a classification difficult. Consequently the following scheme reflects to a large extent the particular prejudices of the writers.

1. *Complete Deprivation.* The most common practice consists in the complete deprivation of food for a specified number of hours or days. An immediate difficulty in the application of this method is the periodicity in eating habits. As Richter (9) has demonstrated, there are rhythms of eating which are closely related to rhythms of gross bodily activity. The responses to food and various other behavioral evidences of motivation are probably more dependent upon the occurrence of hunger-contractions than upon extent of deprivation. Moreover, the periodicity of the hunger-contractions varies considerably from one individual to another. From these considerations it follows that the motivational state of the organism is not adequately defined by such terms as "24-hour hunger" unless the previous feeding habits are also known and specified. In Warner's

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experiment (13), for example, were the rats fed regularly every 24 hours during a training period and then tested at the usual feeding time? Or were they given continuous access to food before the experiment and thus allowed to establish individually varying habits of eating every two, three, or four hours? It is evident that the average motivational measurements for two such groups of rats would differ although each group could be designated by the term "24-hour hunger."

In its simplest form, this method of complete deprivation is unsatisfactory because it does not take into account the rhythmic changes which are associated with the periodic hunger-contractions. These rhythms must be recognized, and they may even be utilized for the control of motivation. In the first place, hunger-contractions, rather than deprivation, may be used as the independent experimental variable. Thus, in Wada's experiment (12), the subjects were undergoing complete deprivation, but this was really a secondary matter. Essentially the experiment attempted to correlate behavioral measurements with the presence or absence of hunger-contractions. In the second place, if the rhythmic changes in the stomach can be temporarily conditioned (and this seems highly probable), the way is open for other and more accurate methods of controlling one of the mechanisms underlying the motivation of hunger.[1] Bousfield and Elliott (2) have suggested one such method, that of establishing a definite eating-rhythm (in the rat) for three weeks prior to experimentation, which should be useful when constancy of motivation over a period of time is desirable. When deprivation of food is excessively prolonged, however, further factors must be taken into account. Morgulis (7, pp. 132 ff.) has shown that metabolic adjustments take place in inanition and that the organism tends to adapt itself to a lowered metabolic level. Bousfield and Elliott (2) found that readjustment to a normal state takes place slowly. Disturbances in the consumption of food after delays of three and one-half and twelve hours in the daily schedule of feeding persisted for two or three days. When the regular feeding was delayed one day, i.e., a fast of two days, rats ordinarily required a week before they again consumed a normal daily ration. The implications of these results

[1]Experiments utilizing and testing some of these methods are in progress in the Harvard Psychological Laboratory.

for experiments in which constancy of hunger is important are that the greater the period of deprivation, the longer must be the intervals between experimentation.

In practice, the writers have found that a single daily feeding of rats is sufficient to maintain normal growth and health after the age of three or four months is reached, provided that sufficient time (one and one-half to two hours) is allowed for the eating of a normal ration. Habits of eating may in this way be conditioned to a high degree of regularity, and the timing of the experimental work may be made in relation to the established rhythm. Different groups may then be tested, for example, in a maze or obstruction apparatus, at the usual time of eating. In this way, time of deprivation may be correlated with behavior uncomplicated by rhythmic effects, since all animals are tested at the times of maximal motivation. In any event, an index of the *constancy* of the internal state, as far as hunger is concerned, may be had by measuring the actual amount of food consumed by the animals during the regular periods of feeding.

2. *Quantitative Deprivation.* A number of experimenters have attempted to control the hunger-motive by feeding an otherwise adequate ration in insufficient amounts. The results of prolonged dietary deficiency (Morgulis' "chronic inanition") have also been studied extensively by the physiologists. Superficially, it appears that this method may be used to produce extreme degrees of motivation by maintaining the regular rhythms of hunger-contractions which tend to become ineffective in prolonged total deprivation. The difficulties, however, are similar to those discussed for the first method. Unless death terminates the experiment, a progressive readjustment takes place to the deficient supply of food until a condition of homostasis is attained. The condition of balance would in turn mean a weaker state of motivation. Valid comparisons under these conditions necessarily depend upon the progress of adjustment to the inadequate diet. At any rate, it seems probable that the physiological changes are too extensive to make possible any direct comparisons of the results of this and other methods.

3. *Qualitative Deprivation.* The control of the hunger-drive through qualitative deficiency consists in the feeding of a diet sufficient in amount but lacking in some essential food substance. Anderson and Smith (1) used this method as well as that of quantita-

tive deprivation. While both methods are effective in increasing activity, it would be difficult to make comparative observations on the constancy and strength of the drives induced under the different conditions.

There are a number of interesting observations on the behavioral effects of qualitative deprivations (11) but relatively few experiments. Probably strong motivation can be produced by this method. However, the relationship to other methods is again uncertain.

4. *Partial Satiation.* A special method of controlling the hunger-motive is implied in the work of Skinner (10). Assuming that the strength of the motivation is indicated by the rate of eating standard pellets of food, and, further, knowing that the rate of eating decreases in a lawful manner while the animal is eating, it may be supposed that motivation can be controlled by allowing the animals to eat until a particular rate of eating has been attained. Several serious objections, however, may be raised against this method. Not only does it imply the possibility of measuring motivation on the basis of a single aspect of behavior but the choice is particularly unfortunate. Bousfield and Elliott (2) found that fasting is effective in decreasing the rate of eating throughout the whole period of eating. The strict application of this finding would therefore force the conclusion that fasting decreases the hunger-drive.

In connection with this method of partial satiation there is also the question as to the extent to which the results are comparable with those obtained by other methods. Carlson (5, pp. 161-198) has shown that the introduction of even a small quantity of food (or other substances) into the stomach will put an end to the hunger-contractions. To the extent, then, that behavior is dependent upon hunger-contractions, the method of partial satiation should show differing results. It seems probable that this method is concerned with the aspect of motivation which Cannon (3, pp. 269-271) designates as "appetite" rather than "hunger." Furthermore, this method can be used only for relatively weak degrees of motivation. It does seem to offer, however, a definitely quantitative way of specifying the strength of a particular group of reflexes related to the internal state of the organism.

5. *Drugs and Glandular Extracts.* All of the methods of controlling hunger which have been presented aim at effecting changes in the internal condition that constitutes the basis of motivation by

various indirect means. A more direct control would consist in the altering of the internal condition by injecting appropriate substances into the blood stream. This method has been utilized in studies of the sexual and maternal drives (8, 14). Up to the present, the method has not been used to any great extent in the control of hunger. Moss (8, p. 140) reports a series of experiments in progress which are intended to determine the effects of insulin and adrenin on the hunger-drive. Obviously, the relationship of results obtained by this method to results obtained by other methods must be determined. It is possible that the use of drugs may furnish, within limits, a quantitative way of controlling motivation which is relatively free from the difficulties involved in the other methods.

Drive and Associated Mechanisms

Misinterpretations and controversies in the literature on drive have been due in part to the assumption that behavioral phenomena, described in terms of reflexes, constitute the essence of motivation. This notion involves the further assumption that the control of drive is the control of behavior. Indices of the reflex strength would thus become indices of the strength of the drive itself. Examples of this are the measures of activity such as crossings of a grid or gross bodily activity. But other measures of behavior, such as the rate of eating, tend to vary inversely with the extent of the deprivation. The situation becomes clearer when it is realized that the behavioral phenomena must be studied in relation to the specific mechanisms which are active at the time. These mechanisms are in turn brought into play through the internal upset which is basic to the drive. Thus the periodic reflex facilitation that appears with fasting indicates a "proprioceptive mechanism" (6). Contractions of the musculature of the stomach give rise to the heightened somatic reflex activity. On the other hand, the inhibition of reflexes relating to the ingestion of food depend upon the arousal of the "sympathico-adrenal mechanism." This latter mechanism is quite generally involved, as Cannon (4, pp. 230-267) has shown in the emergency conditions.

The implications of these facts from the standpoint of the control of hunger are evident. Since these mechanisms are basically different, and since they are not uniformly active throughout the course of

inanition, a knowledge of their characteristics as well as of their relative strength is essential for an adequate interpretation of the behavioral phenomena. An example may be given to illustrate this point. The data obtained by Warner (13) on the relationship between number of crossings in the obstruction apparatus and the extent of food-deprivation indicate a curvilinear relationship. The maximum number of crossings occurred after fasts of two to four days. Thereafter the number steadily decreased. On the basis of our present knowledge it would be difficult to determine whether the final decrease was due to a change in the rôle of the proprioceptive mechanism, on the one hand, or to the increasing effectiveness of the sympathico-adrenal mechanism, on the other. The operation of the latter mechanism would be associated with a decrease in the reward-value of the food consequent upon the inhibition of reflexes concerned with the ingestion and digestion of food.

Conclusions

The solution of these problems waits upon the accumulation of more data. Until some order has been introduced into the basic facts of the primary drives any generalizations must be highly speculative. One of the first steps towards the collection of adequate data must certainly be the standardization of methods of controlling the internal conditions which constitute the core of motivation. Of the various methods which have been discussed, the writers are convinced that the plan of conditioning subjects to a definite spacing of the periods of feeding is open to the fewest objections. For the maximal effects of the drive, experimentation should then take place at the established time of eating.

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LE CONTRÔLE EXPÉRIMENTAL DE L'IMPULSION DE FAIM

(Résumé)

Deux difficultés fondamentales s'opposent à une manipulation quantitative satisfaisante de la motivation. (1) On ne peut contrôler la motivation que d'une façon indirecte puisqu'elle comprend une altération complexe de la condition organique à la base. (2) La motivation comprend souvent nombre de mécanismes distincts plutôt qu'un seul mécanisme facilement contrôlé.

Les diverses méthodes employées dans le contrôle expérimental de la faim sont ici résumées et critiquées à la lumière de ces difficultés. On discute deux des mécanismes qui se trouvent à la base de la faim et on offre des suggestions pour le contrôle plus satisfaisant de ce mobile.

BOUSFIELD ET ELLIOTT

DIE EXPERIMENTELLE KONTROLLE DES HUNGERTRIEBES

(Referat)

Es gibt zwei Grundschwierigkeiten im Wege einer hinreichenden quantitativen Behandlung der Motivierung. (1) Die Motivierung kann nur indirekt kontrolliert werden, da sie eine verwickelte Veränderung des grundligenden organischen Zustandes darstellt. (2) Die Motivierung umfasst häufig eine Anzahl von bestimmten Mechanismen anstatt eines einzelnen leicht kontrollierten Mechanismus.

Die verschiedenen in der experimentellen Kontrolle des Hungers gebrauchten Methoden werden besprochen und im Lichte dieser Schwierigkeiten beurteilt. Zwei der dem Hunger unterliegenden Mechanismen werden besprochen und Vorschläge zur hinreichenderen Kontrolle dieser Motivierung gegeben.

BOUSFIELD UND ELLIOTT

MENTAL FATIGUE UNDER AUTOMATICALLY CONTROLLED RATES OF WORK*

From the Department of Psychology of The University of Chicago

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Investigators of fatigue have pointed out the striking contrast between the curve of muscular work and that of mental work. The former, as studied by means of the ergograph, displays a very rapid decline, reaching a complete breakdown in a few minutes. The latter shows a very gradual decrement which, in most cases, does not terminate in a complete breakdown after several hours. This difference has been accounted for in several ways. To begin with, the elements of the ergographic task are not directly comparable with those of the mental task. The subject is pulling a given mass through a given distance in a given time. A mass is chosen which the subject can just pull. A constant time is allowed per pull. Distance alone is allowed to vary. If a lighter weight were used, or if the time per stroke were lengthened, the subject might keep on longer, even indefinitely. Moreover there is no adequate equivalent in mental work for mass, unless it is difficulty. But the difficulty of a mental task tends to decrease the oftener it is performed, whereas mass remains constant. With regard to the other factor, time per stroke or unit of work, this is held constant in the ergographic task, whereas in mental work the rate of work is voluntarily controlled by the subject, and hence varies.

Unless these differences between muscular and mental work studies are reconciled, there is little justification for the hasty conclusion that the difference in results is due to some fundamental difference in the fatigability of nerve as opposed to muscle.

The purpose of the present investigation was therefore to determine the effect of automatically imposed rates on the amount and

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steepness of the decrement in mental work. Two different problems were involved: (1) The effect of different automatic rates, varying from an extremely slow to an extremely rapid rate. (2) The effect of continuous work for a considerable time at an automatic rate as compared with a rate voluntarily controlled by the subject.

Four different criteria of fatigue were used, speed as measured by responses per minute, accuracy in errors per minute, and number and length of blocks per minute. The last two criteria have been found, in previous studies, to be quite sensitive indices of fatigue (1). A decrement is indicated by an increase in the frequency and length of the blocks. The use of speed, as a criterion, might be questioned, since the purpose of imposing an automatic speed is to hold this variable constant. But if the speed is faster than the subject can maintain, he tends to fall behind, so that responses per minute decrease.

A new type of apparatus had to be devised which would allow for a rapid shift from a subject-controlled rate to an automatic rate of exposure of the stimuli, and which was equipped for registering either manual or vocal responses. A description of this apparatus is given elsewhere (2).

Two types of mental work were used: color-naming and form-naming. The stimuli were painted on a continuous metal film and were exposed serially. Three hundred stimuli were presented before the order repeated itself, so that memory was not involved. The continuity of the work was unbroken throughout the period of work.

Sixty subjects were used, all of them university students with some psychological training. But, of these, only 30 gave complete records in both experiments, so that the data reported are based on these cases. A comparison of the automatic and voluntary performance of the various subjects shows large individual differences. In fact, it suggests the presence of some such trait as that of "load" which is one of the will-temperament traits defined by Downey (3).

The present study can be divided into two main experiments. Experiment I is an investigation of the relation between rate of work and fatigue effects, where rate is automatically controlled. Experiment II is a direct comparison of the fatigue curves of subjects working under automatically controlled and under voluntarily controlled rates.

Experiment I

The Relation between Rate of Work and Fatigue

The mental task was color-naming. In one series, the responses were given vocally, and in the other series, manually. In the manual series, the speeds used approximated the following rates, in responses per minute: 40, 50, 60, 70, 80, 90, 100, 110, 120. But not every subject worked at every speed. Some could not reach a speed above 100 without blocking practically continuously, or else saying color names at random without reference to the stimuli. The vocal range of speeds was slightly higher, ranging from a low of 60 to a high of 140. The fact that the results for the extreme speeds do not represent the entire group of subjects tends to lower the reliability of the points at the ends of the curves in the figures.

The procedure was first to give the subject from five to ten minutes' preliminary practice in following the exposure apparatus, and in making the vocal and manual responses. It was necessary to devote some practice to learning the positions of the five keys used in the manual responses. Next, a five-minute record was obtained for vocal and a five-minute record for manual color-naming at the subject's maximum voluntary speed. His instructions for this performance were to work as fast as possible consistent with accuracy. Then the apparatus was set at the lowest speed, i.e., 40 per minute for manual, or 60 per minute for vocal, depending on which was taken first, and the subject worked at this rate for one minute. At the end of this time, a short rest occurred while the apparatus was being set for the next higher rate. Where the lowest rate was considerably below the subject's natural rate, he was advanced by double jumps until his natural rate was passed. Moreover, if a subject reached a rate where his responses showed absolute confusion, he was not tested beyond this point. Occasionally a reversal would occur, i.e., a case in which less blocks and errors were made at a higher than at a lower rate, but this never held for more than one step on the scale.

In this first experiment, since the work period at each rate was so short, the only criteria of fatigue that could be used were number and size of blocks and number of errors. Figures are presented showing the relation between each of these criteria and the different speeds.

Figure 1 shows the averaged results for number of blocks. The optimum rate is 70 r.p.m., and from this point the curve is positively accelerated, approaching infinity at 130 r.p.m. This means that beyond a speed of 120 r.p.m. the average subject is either totally confused, so that he blocks almost continuously, or else his responses are quite random. Since the instructions given to the subject were designed to establish an accuracy "set" rather than a "set" to respond at all costs, the usual result was a continuous series of prolonged blocks. It will be noticed that there is a slight rise in the curve as the rate is lowered from 70 r.p.m. to 60 r.p.m. It indicates that rates below the subject's optimum may be slightly disadvantageous also.

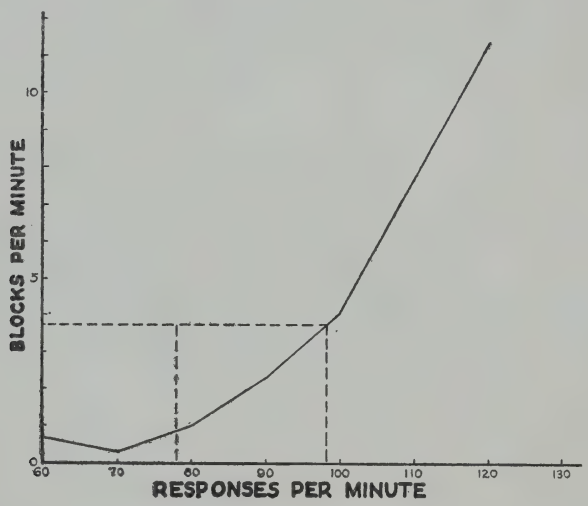


FIGURE 1

Relation between Speed and Frequency of Blocking When Responding Vocally

The dotted lines in Figure 1 are designed to show the relationship between the amount of blocking under automatic and voluntary or self-determined rates of work. The average voluntary rate was 78 r.p.m., the point at which the vertical dotted line cuts the abscissa. At this rate, the number of blocks per minute was 3.7 on the average. When a horizontal line is extended from this point on

the ordinate to cut the curve, then a vertical dropped from the point of intersection to the abscissa meets the abscissa at 98. This means that the subjects had to work at an automatic rate of 98 r.p.m. in order to block as often as they had blocked at a voluntary rate of 78 r.p.m. It indicates that the automatic rate tended to reduce the frequency of blocking considerably, as compared with a self-imposed maximum rate. The theoretical implications of this fact are rather important. While there is, no doubt, some facilitative effect of the external rhythm, and an evener distribution of effort, the main factor appears to be the constant external source of motivation, in place of the internal motivation involved in the self-imposed rate. The exposure apparatus serves as a pace-setter.

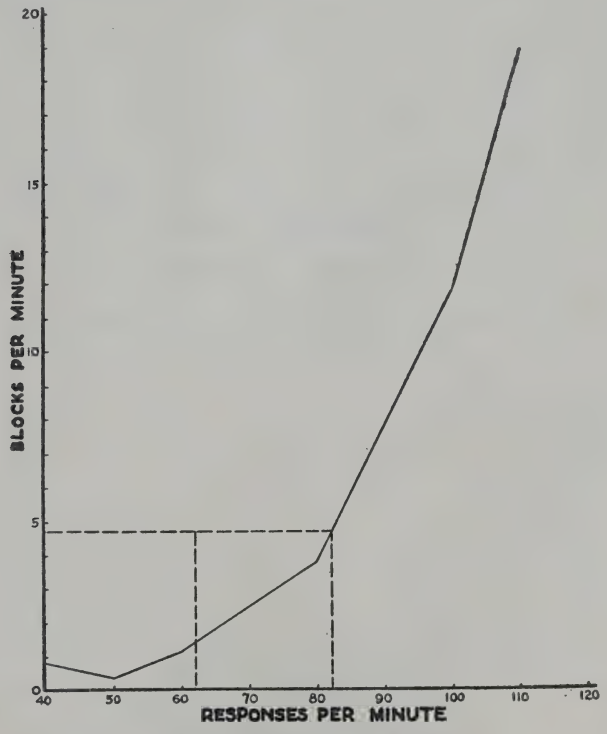
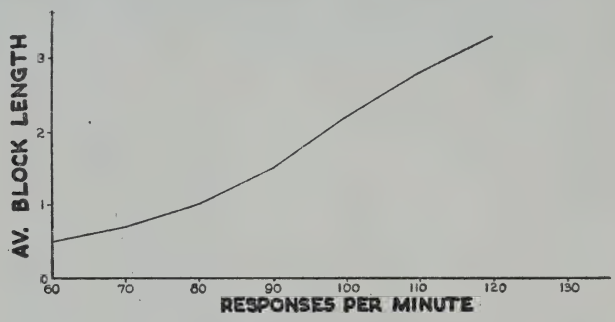


FIGURE 2

RELATION BETWEEN SPEED AND FREQUENCY OF BLOCKING WHEN RESPONDING MANUALLY

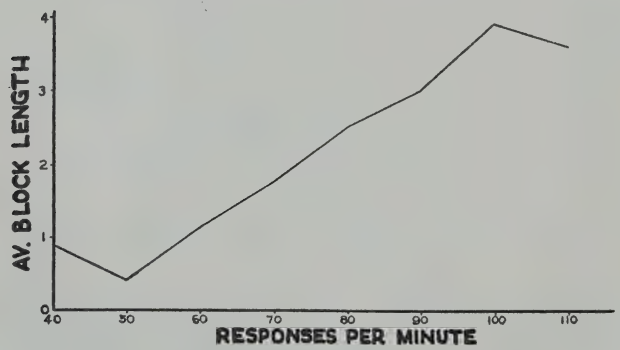
Figure 2 gives the corresponding results for manual color-naming. The picture resembles that for vocal responses. The optimum rate is 50 r.p.m., and from this point the block frequency is positively accelerated, becoming asymptotic to the ordinate at a rate of 110 r.p.m. or thereabout. The dotted lines again show the relation between automatic and voluntary maximum rates as to the frequency of blocks. The voluntary maximum rate was 62 r.p.m., and, at this rate, the average number of blocks was 4.7 per minute. A parallel is drawn from this point to cut the curve, and a vertical line is then dropped to the abscissa, meeting it at about 83 r.p.m.,



AUTOMATIC RATE VOCAL

FIGURE 3

RELATION BETWEEN SPEED AND LENGTH OF BLOCKS WHEN RESPONDING VOCALLY



AUTOMATIC RATE MANUAL

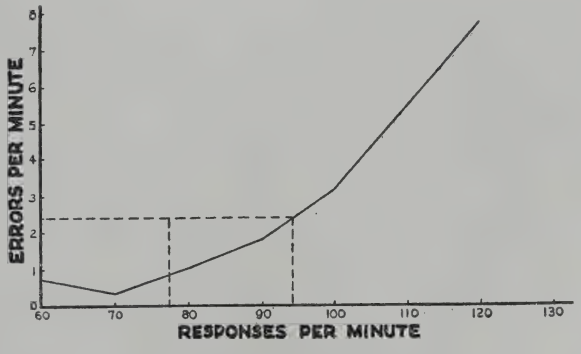
FIGURE 4

RELATION BETWEEN SPEED AND LENGTH OF BLOCKS WHEN RESPONDING MANUALLY

showing that an average subject would have to work at an automatic rate of 83 r.p.m. to have as many blocks per minute as he would have at a voluntary maximum rate of 63 r.p.m.

Figures 3 and 4 show, for vocal and manual responses, respectively, the increase in average length of blocks with increase in rate. Block length is computed in seconds. This has the disadvantage of giving a rigid unit which does not vary with the average response length, and one would expect the blocks to become shorter as the average response became shorter (since a block is defined as a reaction time equal to two or more responses). On the other hand, to translate the graphs from seconds into response times would only enhance the tendency shown in the curves, without producing any change in their direction. The positive acceleration would be more decided. As the graphs stand, it can be seen that the average block length is least at the lowest speed, and that it increases with speed at a slightly accelerated rate until the highest speed is reached. There is an apparent reversal in the manual curve at 110, which is probably due to the subject's tendency at this point to begin responding at random, with a consequent rapid increase in errors, but no increase in blocks.

Finally, in Figure 5, is shown the relation between errors per minute and automatic rate, in the case of vocal color-naming. The optimum rate, for accuracy, is 70 r.p.m., which was also shown to



AUTOMATIC RATE VOCAL

FIGURE 5

RELATION BETWENN SPEED AND ACCURACY WHEN RESPONDING VOCALLY

be optimum for blocking. From this point, there is a positive acceleration which, however, has not rendered the curve asymptotic to the ordinate axis by the time the highest rate is reached. In a sense, accuracy and blocking are complementary. It is impossible for both to approach infinity simultaneously, because an increase in blocks means a decrease in responses, and this in turn means a decrease in possible number of errors.

The dotted lines in Figure 5 have the same function as those in Figures 1 and 2, and show that the average number of errors per minute at a voluntary maximum rate of 78 r.p.m. is 2.5, whereas it requires an automatic rate of 94 r.p.m. to produce the same number of errors per minute.

The results of Experiment I can be summarized in the following general conclusions.

1. Fatigue, as measured by frequency and length of blocks and frequency of errors, increases at an increasing rate with increase in speed of work from an optimum of 70 r.p.m. for vocal and 50 r.p.m. for manual color-naming, to a point of breakdown at approximately 130 for the former and 110 for the latter.

2. When rate is controlled automatically by the rate of exposure of the stimuli, subjects can attain a speed considerably higher than their maximum voluntarily imposed speed before an equal amount of fatique will appear, as measured by the aforementioned criteria.

The periods of work used in Experiment I were extremely short. The question is, will the second conclusion above hold for considerably longer periods of work? Experiment II was designed to answer this question.

Individual Differences. The above conclusions are based on average results. However, there were large differences between the individual subjects as to the size of the discrepancy between their maximum voluntary and maximum automatic rates. For example, the extreme amount of discrepancy was shown by a subject who had a maximum *voluntary* rate of 52 r.p.m. in manual color-naming, and a frequency of blocks of three per minute at that rate; whereas he attained an *automatic* rate of 102 actual responses per minute, with an average block frequency of three per minute. At the other extreme, the smallest amount of discrepancy was shown by a subject who blocked six times per minute at a maximum voluntary speed of

58 r.p.m. manual color-naming, and had an average of five blocks per minute at an automatic rate of 60 r.p.m. This wide individual difference suggests some fundamental difference in the capacity of subjects to attain their maximum performance under voluntary control. Some subjects work at or close to their maximum when under a voluntary set. Others require the added motivation of an external pace-setting stimulus to attain their maximum. Perhaps the latter group are inadequately motivated under the voluntary set; but, if so, it must be a fundamental characteristic of their behavior and hence differentiates them from the adequately motivated group. The trait is not identical with Downey's concept "load," because that defined the difference between the subject's *usual* rate and his rate under speed instructions, whereas the present distinction is between the subject's maximum self-controlled rate under speed instructions and his externally controlled rate. The terms "unpaced" for the former, and "paced" for the latter, better describe the distinction. We can then say that individuals differ in the degree to which they possess the trait, "volitional drive," this trait being defined as the ratio of their "unpaced" maximum speed to their "paced" maximum speed. If this ratio approaches unity, the individual may be said to have a maximum degree of the trait "volitional drive."

Experiment II

The purpose of the second experiment was to determine whether the fatigue effects of "paced" work are greater or less than those of "unpaced" work when the work period is lengthened to 15 minutes or a half hour. Longer periods were found impracticable because of the sensory fatigue which causes blurring of the stimuli, especially at rapid rates.

The experiment as carried out involves two separate sections, in the first of which colors were used as stimuli, and in the second, geometrical forms. The results for colors will be given first. There were two conditions which were compared directly. Under one, the subjects worked at their self-controlled maximum for 15 minutes without rest. Under the other, they worked with the exposure apparatus set at a rate which had been found to give about the same initial frequency of blocking as that self-controlled rate in the preliminary experiment. This second condition was the "paced" condi-

tion. We would naturally expect the rate to be faster than the "unpaced" rate, and it was with some subjects, but the average for all was about the same due to practice, received during Experiment I and during the "unpaced" part of Experiment II. The order of taking the conditions was alternated from subject to subject. The results were analyzed to determine (1) the amount of decrement in responses per minute during the 15 minutes, (2) the increase in number of blocks per minute, and (3) the increase in size of blocks per minute. These results are given in Table 1. In order to

TABLE 1

AVERAGE FATIGUE CURVES FOR FIFTEEN-MINUTE COLOR-NAMING

Responses per minute (by min.)															
Paced	87	82	79	79	79	78	77	77	77	76	77	76	76	76	74
Unpaced	86	80	80	79	78	78	78	78	79	80	77	75	76	77	77
Blocks per minute (by min.)															
Paced	2.1	3.6	3.8	4.1	3.7	3.7	3.7	3.9	3.7	3.8	3.7	3.0	3.2	3.5	3.7
Unpaced	2.7	3.8	3.7	4.4	5.0	4.7	5.0	4.7	4.4	4.7	5.2	6.2	6.6	5.7	6.0
Length of blocks in millimeters (by min.)															
Paced	3.6	4.6	4.4	4.2	4.2	3.6	3.9	4.4	4.9	4.4	4.3	4.4	4.3	4.6	4.7
Unpaced	4.6	4.8	4.8	4.8	5.2	5.1	5.0	4.9	5.3	4.9	5.6	5.7	5.1	5.6	4.8
Errors per minute (by min.)															
Paced	2.0	2.5	2.0	2.9	3.3	2.8	2.5	2.8	2.0	3.0	2.5	2.5	2.7	2.5	2.5
Unpaced	2.6	3.2	3.7	3.0	3.4	3.2	3.5	2.7	2.9	3.6	4.1	3.1	2.6	2.3	3.0

compare the relative decrements under the two conditions, we can compute the size of the decrements by determining the difference between the average of the first five minutes and the last five minutes of each work curve. By this method, we obtain, for responses per minute, a decrement of 5 r.p.m. for the "paced" and 4.5 r.p.m. for the "unpaced" condition. It is evident that the two conditions give no significant difference in the amount of speed decrement. But, for blocks per minute, there is a decided difference. The "paced" condition shows no increase in blocks per minute, whereas the "unpaced" condition shows an increase from 3.9 to 6.0, which is an absolute increase of 2.1 blocks per minute, and a percentage increase of 54 per cent. As to length of blocks, the "paced" condition shows an increase from 4.2 to 4.5, or 0.3 millimeters, whereas the "unpaced" condition shows an increase from 4.76 to 5.36, or 0.6 millimeters. With regard to errors, there is no decrement in either condition, and hence no significant difference between the two conditions.

This leads to the conclusion that the "paced" condition is, by certain criteria of fatigue, i.e., frequency and length of blocks, a less fatiguing condition than the "unpaced." It suggests that the decrement which occurs in the "unpaced" work is, in part, at least a breakdown in the general facilitative process which is commonly called volitional control or drive. The stimulus-response elements involved in the operation itself are capable of continuing to react at the same rate, but they lack adequate reinforcement from the general regulative and facilitative mechanism.

The second part of the experiment differs from the first only in using geometrical forms in place of colors. Five different forms were used: circle, square, triangle, diamond, and oval. They were painted on the film in solid black on a white background. The same two conditions were used as before, i.e., a "paced" and an "unpaced" condition. The work period was lengthened to a half hour. The results of this experiment do not differ materially from those with colors, except that the strangeness of the forms caused a greater amount of practice effect with the result that the half-hour work curve shows an increment instead of a decrement in performance in the "unpaced" condition. However, the reduction in length and frequency of the blocks is greater for the "paced" condition.

The results of Experiment II can be summarized as follows.

Subjects working at a "paced" rate show a smaller increase in frequency and length of blocks than when working at an equivalent "unpaced" rate, at least for the types of mental task and lengths of work period used in this investigation.

Several limitations appeared in the course of the study which reduce the significance of the results and should be avoided in further investigations along this line.

1. The differences between individuals, both in their reaction to the "paced" condition and in their flexibility of rate, suggest that the whole problem could be more profitably approached as a problem in individual differences.

2. The large practice effects suggest that a paired group procedure would have differentiated better between the effects of the two conditions in this experiment.

3. We still do not know to what extent the benefit of the pacing was due to the regularity of the rhythm, and to what extent it was due to the reinforcing effect of an external excitant.

With regard to the general question asked at the beginning of this article, i.e., whether the failure to get as rapid fatigue in mental work as in ergographic work is due to the voluntary rate permitted in mental work, we have answered in the negative. Imposing a mechanical rate on mental work does not produce any more rapid breakdown, but rather a *less* rapid one.

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LA FATIGUE MENTALE SOUS DES VITESSES DE TRAVAIL AUTOMATIQUEMENT CONTRÔLÉES

(Résumé)

Dans cette étude il s'agit (1) de la relation entre la vitesse du travail et de certains phénomènes de fatigue, principalement la fréquence et la grandeur des périodes qui s'y trouvent; et (2) de la fatigue relative sous une vitesse automatiquement ou volontairement contrôlee, c'est-à-dire le travail "gouverné" ou "non gouverné." Les tâches mentales ont été de nommer les couleurs et les formes, les stimuli étant présentés en forme de séries au moyen d'un appareil spécial d'exposition. Les sujets ont été trente étudiants universitaires. Ils ont répondu ou vocalement ou manuellement à des vitesses variant de 40 à 140 stimuli par minute. On a constaté que la fréquence et la longueur moyennes des périodes et des erreurs ont augmenté à une vitesse augmentée comme la vitesse du travail a augmenté d'un optimum de 70 pour l'action de nommer les couleurs vocalement et de 50 pour celle de les nommer manuellement jusqu'au point de cessation à approximativement 130 pour celle-là et 110 pour celle-ci. La vitesse "gouvernée" peut dépasser la vitesse *maxima* "non gouvernée" considérablement avant de produire une plus grande fréquence des périodes et des erreurs. Quand les sujets ont travaillé continûment plus tard pendant quinze minutes ou une demi-heure, l'effet de fatigue s'est montré plus grand pour la condition "non gouvernée." Les individus ont beaucoup différé quant à la différence entre leurs vitesses maxima "non gouvernées" et "gouvernées." On suggère le nom "impulsion volitionnelle" pour ce trait.

Bills et Shapin

GEISTIGE ERMÜDUNG UNTER AUTOMATISCH KONTROLLIERTEN SCHNELLIGKEITEN DER ARBEIT

(Referat)

Diese Untersuchung beschäftigt sich mit (1) dem Verhältnis zwischen der Schnelligkeit der Arbeit und gewissen Ermüdungserscheinungen, hauptsächlich mit der Häufigkeit und Grösse der stattfindenden Versperrungen; und (2) mit der relativen Ermüdung unter automatisch oder willkürlich kontrollierter Schnelligkeit, d.h. markierter und nichtmarkierter Arbeit. Die geistigen Aufgaben waren Farbenbenennung und Formbenennung. Die Reize wurden in einer Reihenfolge vermittelst eines besondren Darbietungsapparats dargeboten. Die Versuchspersonen waren dreissig Universitätsstudenten. Sie reagierten entweder wörtlich oder mit der Hand in Abständen von 40 bis zu 140 Reizen pro Minute. Die Durchschnittshäufigkeit und Länge der Versperrungen und Irrtümer, wie festgestellt wurde, nahmen zu, wie die Arbeit von einer Höhe von 70 für wörtliche und 50 für Handfarbenbenennung bis auf den Punkt der Zusammenbrechung bei ungefähr 130 für die erstere und 110 für die letztere zunahm. Die markierte Schnelligkeit kann die maximalnichtmarkierte Arbeit beträchtlich überschreiten, bevor sie eine grössere Häufigkeit der Versperrungen und Irrtümer hervorbringt. Wenn die Vpn. später fortlaufend für 15 Minuten oder eine halbe Stunde arbeiteten, war die Ermüdungswirkung grösser als bei der nichtmarkierten Arbeit. Die Vpn. unterschieden sich bemerklich in bezug auf die Verschiedenheit zwischen ihren nichtmarkierten und markierten Maximalschnelligkeiten. Das Wort "Willenstrieb" wird für diese Eigenschaft vorgeschlagen.

BILLS UND SHAPIN

THE RELIABILITY AND VARIABILITY OF THE TESTS USED TO MEASURE THE ACCOMMODATIVE RESPONSE AT THE NEAR POINT*[1]

From the Department of Psychology of the University of Minnesota

JACK I. KURTZ

INTRODUCTION

Many investigators of the accommodative function are agreed that different results are obtained by testing the same function using different types of tests; hence the findings of one investigator are not at all comparable with those of another. We have long suspected that this is also true when the same function is examined by one person several times, using the same type of test each time. Many clinical workers have been confronted with this difficulty. Howard (8) found that, by using the ordinary methods for testing accommodation of candidates for the Aviation Corps, his results were most unreliable.

There are a number of factors contributing to this unreliability. Some of them are:

1. Variation in size of test object.
2. The difference in motive of examination.
3. Variation in speed of approach of test object.
4. Difference in degree and type of illumination.
5. Variation in legibility of test type due either to the size of the test type or to the difference in reflection coefficient of the test chart.
6. Difference in position from which the reading distance is computed.

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[1]We wish to acknowledge our indebtedness to Professor W. T. Heron of the Psychology Department and to Dr. E. Lyon, Dean of the Medical School, for their helpful suggestions and criticism, and to the American Optical Company, Southbridge, Massachusetts, for their kindness in loaning us the equipment used in this study. We are especially indebted to Dr. M. A. Tinker for his many valuable suggestions and kind cooperation which made it possible for us to carry out this study in his laboratory.

7. The personal equation difference of both the examiner and the subject.

8. Variation in the way the patient is instructed.

9. The size of retinal image increases as the test type is approached to the eye, the retinal images are more diffused and appear larger; hence these images are more easily interpreted mentally than the more distinct but smaller images. The subject is therefore able to read at a point nearer than that for which accommodation is enforced.

10. As the reading material approaches the eye the lids partly close and thus the narrowing of the palpebral fissure is equivalent in action to a stenopaic slit and is a material factor in the reading ability, especially in cases of astigmatism.

11. When concave lenses are used to test accommodation the size of the retinal images is reduced considerably and thus affects the amount of accommodation the subject is to exert at a given distance.

Review of the Literature. In reviewing the literature we find that several observers were aware of this difficulty and tried to attack this problem from different angles. Howard (8), Lee (13), Thorne (21), and Blatt (2) have devised various instruments to measure accommodation, with which they claim they obtained more accurate results than before. However, they fail to furnish any data to substantiate their claims. They do not give any data about the results they obtained before nor do they compare these data with the findings obtained with their new instruments. The tests were all subjective. A. J. Cross (3), one of the pioneers in modern optometry, realizing pitfalls of the subjective accommodative tests, set out to discover an objective method. In 1901 Cross gave to the world "Dynamic Skiametry," an objective method for testing accommodation. At first the "Dynamic Skiametry" technic, like everything else that is new, was crude. Many optometrists tried to use this method in their ocular examinations, as advocated by the discoverer, but some of them soon had to abandon it, because, while the findings with this new method appeared to be correct theoretically, they could not be utilized practically. It was not until 1917 that Sheard (17), through a series of experiments, was able to arrive at conclusions which helped in the interpretations of the dynamic skiametry findings and thus make them practical for clinical application. A num-

ber of other workers in optometry, notably Tait (20), Bestor (1), and McFadden (15), have through their studies and experimentations further developed and simplified the interpretation of dynamic skiametry.

The Problem. From the above discussion we can see the importance of a more accurate knowledge of the behavior of the accommodative apparatus. Thus the following problem, "The Reliability and Variability of the Tests Used to Measure the Accommodative Response," presents itself. In investigating this problem we are interested to find some information about these points.

1. What is the minimum amount of negative accommodation, i.e., the amount of accommodation used at the near point?
2. What is the maximum amount of negative accommodation?
3. Is there a variation in the findings when the tests are made monocularly or binocularly?
4. What agreement is there in the negative accommodation findings using different tests?
5. Which one of these tests is most or least reliable and variable?

The Experiment

1. *Laboratory and Illumination.* The experiment was carried out in a laboratory room at the Psychology Department at the University of Minnesota which was provided with all the necessary equipment needed to perform a complete refraction quite similar to the type performed clinically by any modern refractionist. The illumination was kept constant between 12 and 18 foot-candles and was reduced somewhat only during the time when the dynamic retinoscopy tests were performed.

2. *Apparatus and Equipment.* The apparatus used in making the different tests in this experiment consisted of:

a. Phoroptor. This instrument was kindly loaned to us for this study by the American Optical Company, Southbridge, Massachusetts, and it consists of batteries of (*a*) convex spherical lenses, (*b*) concave spherical lenses, (*c*) concave cylinders, (*d*) prisms, (*e*) two sets of rotary prisms, (*f*) two madox rods as illustrated in Figure 1.

b. Dynamic skiascope. This instrument is used to make the

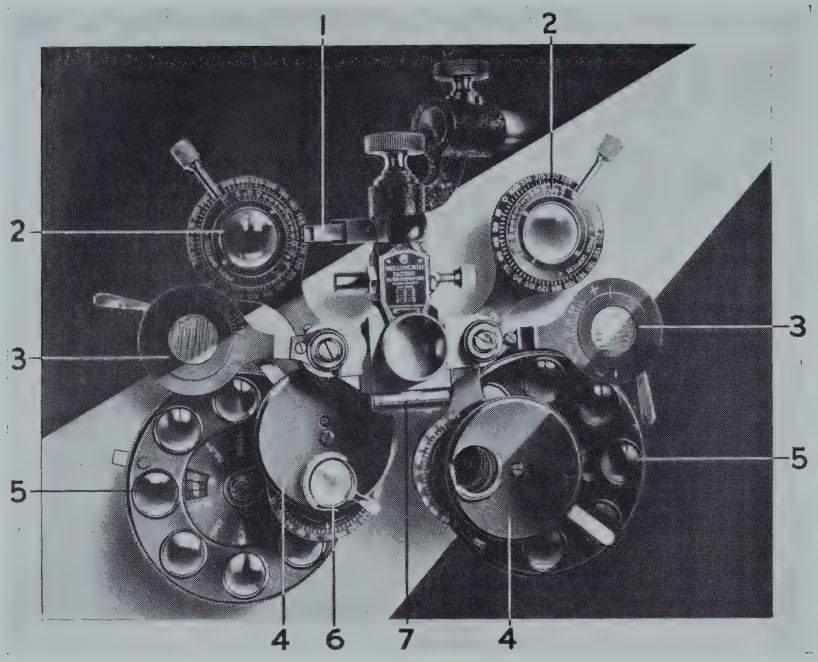

FIGURE 1

PHOROPTOR

(1) Part for attaching bar and test object carrier, (2) Rotary prisms, (3) Maddox rods, (4) Battery of cylindrical lenses, (5) Battery of spherical lenses, (6) Cross cylinders, (7) Spirit level for leveling of the instrument.

skiascopic tests, to determine objectively the amount of negative accommodation. This instrument, illustrated in Figure 2, may be used in connection with current control, so as to enable one to regulate the amount of current to be used, or with a battery handle.

c. Cross cylinder auxiliary lens set.

d. Test cards. A reading card with very fine type was used. The card was about 140 mm. x 110 mm. in size. One side had one column of type 30 mm. x 20 mm. in size and was used to make the binocular test where both accommodation and convergence were used. This column of type was doubled through the use of a prism placed base down over the right eye. The other side of the same card

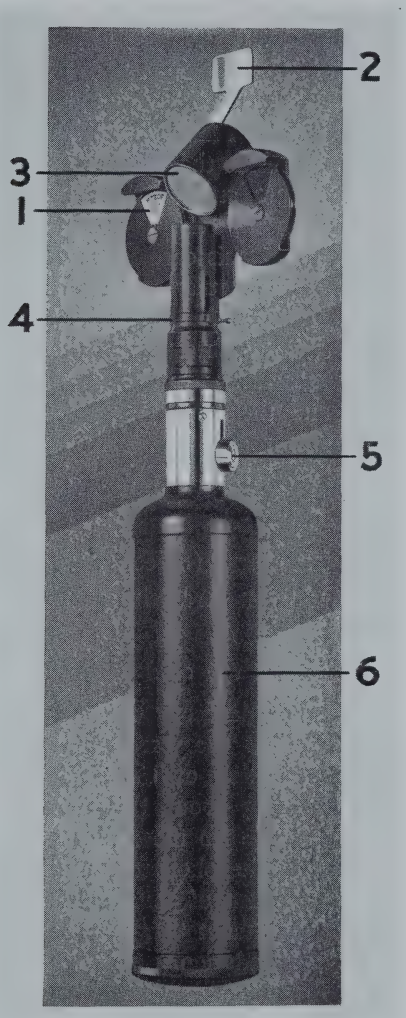

FIGURE 2

TAIT DYNAMIC SKIASCOPE

(1) Movable fixation target, (2) Forehead rest, (3) Reflecting mirror, (4) Collar for reflecting light upon fixation target, (5) Light control, (6) Battery handle

had two columns of type 30 mm. wide, 97 mm. high, and 55 mm. apart. The right column was used for the right eye and the left column for the left eye. In this test each eye was tested separately; accommodation was used but not complete convergence.

e. Cross cylinder card. This card was the same size as the one just described. On the one side there were vertical lines, 25 mm. in length, spaced 1.5 mm. apart, and five lines of the same size passing through the center horizontally, the same distance apart, forming a cross. On the other side of this card there were two such crosses, 60 mm. apart. For the binocular testing, that side of the card was used which had only a single object; for monocular testing the other side, which had two test objects, was used.

All tests in this investigation were performed first monocularly and then binocularly and the "minimum and maximum" amounts obtained were properly recorded.

3. *Methods Used for Testing Negative Accommodation.*

a. Clinical method.

1) Dynamic retinoscopy (objective test).

2) Plus lenses and test card with columns of fine type to be later called the reading card.

3) Cross cylinder and test card with cross lines.

In making the dynamic retinoscopy test monocularly, the left eye was occluded while the right eye was tested and vice versa. We first tried to get neutralization of the shadow. The amount of spherical lens taken to bring about this neutralization point was recorded as the "minimum," then more plus was added by turning the proper lens battery in the phoroptor until a point of reversal was reached; the total amount of spherical lens required to bring reversal of the shadow movement was recorded as the "maximum" amount of negative accommodation.

The test with the "reading card" was also made at 1/3 of a meter. One eye was occluded while the other was tested during the monocular test. Both eyes were uncovered during the binocular test. (This procedure was carried out when we performed any of the tests in this experiment.) Plus lenses were placed before the eye under the test until the least amount of blur of the type was noticed.

This was recorded as the "minimum"; then more lenses were added until the print was so blurred that it could not be read. This was recorded as the "maximum" amount of negative accommodation.

The test with the "Cross cylinder" card was performed monocularly and binocularly as described above. A "Cross cylinder lens" of the combination —0.50 cylinder axis combined with a 90 +0.50 cylinder axis 180 was placed before each eye under the test. The subject was asked to note if the vertical and horizontal lines on the test card appeared the same shade of darkness. If the horizontal lines appeared darker (as they did in most cases) plus spherical lenses were added until the two sets of lines appeared equal. This amount was recorded as the "minimum." Then more plus was added until the "vertical lines" appeared darker. The total amount of plus lens before the eye was recorded as the "maximum" amount of negative accommodation.

4. *Methods of Work.*

a. Subjects. Of the 26 subjects used, 21 were graduate students at the University of Minnesota and five were not students. Of this group two were optometrists, two were their assistants who understood something about refraction, and one was a school teacher. The whole group displayed an intelligence which was above the average and made very good subjects for this work. All subjects were given a thorough refraction and only subjects with normal eyes or whose refractive error could be properly corrected were accepted.

b. Directions. Each subject was given complete instructions about the different tests, and several trial tests were made first, in order that the subject might know definitely what was expected of him, before the actual experimental test was started. When the reading test card was used in the negative accommodation tests, the subject was instructed to get definitely fixed in his mind as to how clear the type appeared so as to be able to tell when the "least amount of blur" appeared. He was then to report "stop." He was next to report "gone" when the type was so blurred that he could no longer read it.

When the Cross cylinder card was used, the subject was instructed to observe closely the appearance of the vertical and horizontal

sets of lines. If both sets appeared alike, then this was recorded as the "minimum" and lenses were added until the vertical lines appeared darker. The observer was then to report "gone"; this was recorded as the "maximum."

c. Recording data. A special set of charts was prepared and all data were recorded on these charts. One chart was used for each of the "six different and complete sets." For instance dynamic skiametry is one unit of the six different complete tests. This one unit is in itself made up of four separate tests, namely, right-eye monocular test, left-eye monocular test, right-eye binocular test, and left-eye binocular test. In each instance a test was made for the "minimum" and the "maximum." The same holds true for the other two units included in the "complete set of tests." There were 22 individual tests made during the period in which the six different sets of tests were given. (The tests are described on pages 358-360. The number of subjects tested, the number of tests made, the number of test periods, etc., are given in Table 1.)

TABLE 1

STATISTICAL DATA

1.	Total number of subjects	26
2.	Average number of weeks each subject was kept under observation	9
3.	Average number of "complete sets of tests" made on each subject	26
4.	Total number of "individual tests" made on each subject during one complete test	22
5.	Average number of "individual tests" made on each subject during the 26 complete tests	572
6.	Total number of individual tests made on all subjects during the entire period of the investigation	14,872

RESULTS

1. *Theoretical and Practical Considerations.* It may be well to discuss further the theoretical and practical considerations regarding the different tests used in this study of the reliability of the near-point ocular tests. The problem naturally resolves itself into two distinct parts, namely:

1. Negative accommodation both monocular and binocular.
2. Amplitude of accommodation both monocular and binocular.

We shall discuss the negative accommodation tests and the results.

Three different methods of testing each function were used as

explained and outlined on pages 354 and 355. In view of the fact that these two functions play such an important part in our ocular economy, it is of importance that one know something more definite and certain about the measuring rod we employ to measure these functions. Supposing that three different refractionists, *A*, *B*, and *C*, examined the negative accommodation of a patient *X*. Now if *A* used dynamic retinoscopy, *B* used the reading chart and plus lenses, and *C* used the Cross cylinder, will *A*, *B*, and *C* get the same results; if not to what extent may we expect these results to vary? These are the questions we hope to answer in this study. It is a purely clinical question, but of extreme importance. Suppose *A*, in testing his patient, using dynamic retinoscopy, finds +2.50 D.S., *B* using the reading card and plus lenses finds +2.00 D.S., and *C* using the Cross cylinder test finds only +1.00 D.S. While there might be a resemblance between the formulae which *A* and *B* prescribe for this patient, there can most certainly be no resemblance in the prescriptions which *A* and *C* may give to the patient. It may be argued by our theoreticians that, if there is such a discrepancy between the findings of *A* and *C* or *B* and *C*, they are each testing a different function; but by advancing such an argument one overlooks a very important fact, namely, that under each test the eye is made to perform the same visual function, at the same distance (33 centimeters), and under the same illumination. Hence the accommodative effort required by the eye to perform this function should be the same under each test. If the amount is different in each case, then we may be justified in assuming that the difference is due to some element of variability in these tests.

Before we introduce statistical material about any of the tests, let us try to get a clearer understanding of the nature of these tests. According to Sheard (17), Cross contended that dynamic skiametry is an objective test of negative relative accommodation. Cross (3), as the inventor of dynamic skiametry, advanced the theory that "a muscle in action and abnormally taxed will accept assistance more readily than a muscle at rest and when normally enervated and normally functioning." He further contended that there is a more or less fixed relationship between accommodation and convergence, the ratio being 3:1, and, under normal conditions, the eye will relax only such an amount of accommodation, which will make it possible

to maintain the harmonious accommodative convergence relationship. Sheard (17) questions this statement by Cross and contends that the ratio between accommodation and convergence is not so rigid as Cross contends, but that there is rather a lag of accommodation to the extent of 0.75 D. In discussing dynamic retinoscopy, Davis (4) holds that there is no difference between the dynamic retinoscopic tests and negative relative accommodation. He arrived at this conclusion from the results of examining some 4500 cases.

The Cross cylinder test (the technic of its application was described in the preceding pages) is used by many to determine the amount of accommodative effort used at the reading point. Hence the findings of this test should correspond to the findings of dynamic retinoscopy and the reading card and plus lenses test, if these three different methods test the same function. That the results are not the same will be seen from an examination of the experimental evidence presented in the following pages.

The Odd-Even Reliability of the Negative Accommodative Tests. A study of the reliability of the different tests used in this investigation of negative accommodation shows that nearly all tests have a reliability coefficient of .99+ with the exception of the Cross cylinder which has correlation of .92 for the minimum and .98 for the maximum, as is shown in Table 1*a*. This indicates that the negative accommodation tests are reliable.

TABLE 1*a*

COEFFICIENT OF RELIABILITY AND (*B-Sr*)
BROWN-SPEARMAN ODD-EVEN RELIABILITY OF THE NEGATIVE ACCOMMODATION

Types of tests Negative accommodation	Monocular				Binocular			
	Minimum		Maximum		Minimum		Maximum	
	r	*B-Sr*	*r*	*B-Sr*	*r*	*B-Sr*	*r*	*B-Sr*
Dynamic skiametry	.99	.99	.99	.99	.99	.99	.99	.99
Plus lens	.99	.99	.99	.99	.99	.99	.99	.99
Cross cylinder	.92	.96	.98	.99	.98	.99	.98	.99

Table 2 represents the results obtained from testing 26 subjects; each subject was tested on the average 52 times. In these tables are recorded, in diopter values, the averages, range, and standard deviation obtained from testing the negative accommodation by using the three different tests already described, namely: (1) dynamic retin-

TABLE 2

MONOCULAR NEGATIVE ACCOMMODATION

The values in this table are the averages for the entire group of 26 subjects comprising 1352 tests.

	Values in diopters						Percentage coefficient of variation	
	Average		Range		S.D.			
	Mi	Ma	Mi	Ma	Mi	Ma	Mi	Ma
Types of tests	A	B	C	D	E	F	G	H
Dynamic skiametry	2.70	3.00	1.00	0.89	0.24	0.25	12.17	9.79
Plus lenses and reading card	2.75	3.58	1.97	1.64	0.50	0.43	27.16	13.81
Cross cylinder	1.16	1.43	1.20	1.92	0.29	0.44	77.83	56.30

oscopy, (2) plus lenses and reading card, (3) Cross cylinder test.

In the columns A and B of each table are recorded the averages of each subject, in columns C and D, the range, in columns E and F, the standard deviation (S.D.), and in columns G and H, the coefficient of variability.

From studying the figures in Table 2, we find that the results from the dynamic skiascopy test are of considerable uniformity in the amount of the difference between the minimum and the maximum of the averages, range, and S.D., the coefficient of variation being especially low. We may therefore conclude that the monocular dynamic skiametry test yields highly constant results.

The plus lens reading test in Table 2 shows considerable variation between the minimum and maximum of the averages, the difference being 0.83 D. The range shows equal variation. The difference for the group is 0.33 D, and there is a difference of 13.29 per cent between the minimum and maximum of the coefficient of variation. On the whole, there is more variation in this test than in the dynamic skiametry test. We may conclude from this that the plus lens reading card test for negative accommodation, because of its greater variability, yields results less constant than the dynamic skiametry test.

The Cross cylinder test in Table 2 shows considerable variability, especially in the findings of each individual subject. Some investigators claim that the Cross cylinder test can be performed only upon

very intelligent subjects. Tait (20) particularly stresses this view. The patient's intelligence, we grant, is an important factor in making any subjective test, especially the Cross cylinder test. The point most striking is the case of one subject, a student of the graduate school with superior intelligence and ability. Yet in the Cross cylinder test this student had a minimum of 0.00 D and a maximum of 1.34 D, while the dynamic test showed a 2.58 D minimum and a 2.85 D maximum, and in the second type of test, namely the plus lens reading card, the minimum was 3.00 D and the maximum, 3.28 D.

It is of particular importance that a proper evaluation of the Cross cylinder test be made, because great stress has been laid on the value of this test from a clinical and diagnostic standpoint. The writer (11), in a study of the Cross cylinder test, found that in 86 per cent of the cases the results on Cross cylinder tests were considerably lower than on the dynamic retinoscopy test. The reason for this discrepancy may be found in the fact that the Cross cylinder test is essentially an astigmatic test, in which the interval of sterum (19) is the underlying principle, and, while it may be a sensitive test for astigmatism, it does not serve as a valid subjective test for negative accommodation.

TABLE 3

MONOCULAR NEGATIVE ACCOMMODATION

The values in the table are the averages for the entire group of 26 subjects comprising 1352 tests.

Types of tests Monocular	Average of minimum and maximum in diopters	Coefficient of variation in percentage
Dynamic retinoscopy	2.85	10.83
Plus lenses and reading card	3.16	20.51
Cross cylinder	1.29	67.06

Table 3 is a summary of the averages of the minimum and maximum values in columns A, B, G, and H of Table 2. A glance at column 2 in Table 3 shows plainly that the coefficient of variability of the dynamic retinoscopy test is 10.83, that of the plus lens test 20.51, and that of the Cross cylinder test, 67.05 per cent. This last-mentioned test is seven times as variable as the first test and three times as variable as the second test.

TABLE 4

BINOCULAR NEGATIVE ACCOMMODATION

The values in the table are the averages for the entire group of 26 subjects comprising 1352 tests.

Types of tests	Values in diopters						Percentage coefficient of variation	
	Average		Range		S.D.			
	Mi A	Ma B	Mi C	Ma D	Mi E	Ma F	Mi G	Ma H
Dynamic retinoscopy	2.39	2.68	1.00	0.93	0.27	0.25	12.50	10.51
Plus lenses and reading card	2.30	3.05	1.87	1.73	0.50	0.40	24.98	14.45
Cross cylinder	0.72	1.38	1.36	2.00	0.36	0.47	0.84	43.35

In Table 4 are recorded the binocular values of the negative accommodation. The same three tests were used, namely:

1. Dynamic retinoscopy.
2. Plus lens and reading card.
3. Cross cylinder.

It will be observed in Table 4 that in the dynamic retinoscopy test the difference between the minimum and maximum averages is again small, being 0.29 for the entire group. For the range the difference is 0.7 and the difference in the coefficient of variation is two per cent between the minimum and maximum respectively.

In the same table the difference between the minimum and the maximum of the averages of the plus lens and reading card is small, but somewhat larger than the values in the dynamic test. The difference for the entire group is 0.75 D, the difference in range between the minimum and the maximum for the group is 0.14 D, and the difference in the coefficient of variation is 10.53 per cent.

In Table 4 the difference between the minimum and the maximum of the Cross cylinder is large, as it was found to be in Table 2. The variation in the range of the Cross cylinder test is quite large in a number of the subjects. The difference for the group between the minimum and the maximum is 0.64. The coefficient of variation for this test is very high, for the minimum, 84.87 per cent and for maximum, 43.35 per cent, a difference of 41.52 per cent.

In Table 5 are given the average values of the minimum and

TABLE 5

AVERAGE VALUES OF THE MINIMUM AND MAXIMUM AMOUNTS OF THE NEGATIVE ACCOMMODATION OBTAINED FROM THE THREE DIFFERENT TESTS AND THE COEFFICIENTS OF VARIATION

Types of tests Binocular negative accommodation	Average of minimum and maximum in diopters	Coefficient of variation of the three tests in percentage
Dynamic retinoscopy	2.45 D	11.51
Plus lenses and reading card	2.67 D	19.71
Cross cylinder test	1.05 D	64.06

maximum of the averages as given in columns A and B, as well as the average of the minimum and maximum of the coefficient of variation. In columns G and H the comparison of values can be made at a glance, summarizing the results of the binocular dynamic retinoscopy, plus lens, and Cross cylinder tests of negative accommodation. The minimum and maximum values of the averages and coefficient of variation are combined and their averages given in columns 1 and 2. The figures in column 2, Table 5, reveal points of interest similar to those found in Table 3. Here we find again that the coefficient of variation of the dynamic retinoscopy is 11.51, of the plus lens test, 19.71, and of the Cross cylinder, 64.06. This is nearly six times as variable as the first test of this group and more than three times as variable as the second test.

We have found no reference in literature to the difference in amount between monocular and binocular negative accommodation. It appears, from comparison of the averages of the minimum and maximum values for the entire group obtained from the use of the three different tests of negative accommodation, that the monocular

TABLE 6

DIFFERENCE BETWEEN MONOCULAR AND BINOCULAR NEGATIVE ACCOMMODATION

	Values given in diopters		
Types of tests Negative accommodation	Monocular Average of the minimum and maximum averages	Binocular Average of the minimum and maximum averages	Difference between monocular and binocular
Dynamic retinoscopy	2.85	2.54	0.31
Plus lenses and reading card	3.16	2.67	0.51
Cross cylinder	1.29	1.05	0.24

values are somewhat higher than the binocular. In Table 6 we combine the figures from columns 1 and 2 of Tables 3 and 5. It is shown in Table 6 that the amount of monocular negative accommodation is greater than the binocular by 0.31 D when dynamic retinoscopy is used, 0.51 D with the plus lens test, and 0.25 D with the Cross cylinder test.

This general agreement of the three different tests would tend to indicate that monocular negative accommodation is greater than binocular in the amount ranging from 0.25 D to 0.50 D.

In Table 6*a* is given the reliability of the negative accommodation

TABLE 6*a*

THE RELIABILITY OF THE DIFFERENCES BETWEEN THE AVERAGES OF THE NEGATIVE ACCOMMODATION

Types of tests Negative accommodation	Monocular Minimum Diff. of mean	S.D.$_{diff.}$	D S.D.$_{diff.}$	Monocular Maximum Diff. of mean	S.D.$_{diff.}$	D S.D.$_{diff.}$
Dynamic skiametry vs. plus lens	.69	.99	.69	.15	.74	.02
Dynamic skiametry vs. Cross cylinder	2.06	.86	2.38	1.54	.87	1.74
Plus lens vs. Cross cylinder	1.37	.66	2.65	1.70	.72	2.34
Dynamic skiametry vs. plus lens	.01	.81	.01	.49	.67	.72
Dynamic skiametry vs. Cross cylinder	1.53	.66	2.31	1.13	.70	1.60
Plus lens vs. Cross cylinder	1.54	.83	1.84	1.62	.81	2.00

TABLE 6*b*

The coefficients of correlation between the different negative accommodation tests and their P.E. are given in this table for the minimum and maximum values of the monocular and binocular findings.

Tests correlated	Monocular Minimum r	P.E.$_r$	Monocular Maximum r	P.E.$_r$	Binocular Minimum r	P.E.$_r$	Binocular Maximum r	P.E.$_r$
Dynamic skiametry vs. plus lenses	.27	.13	.64	.07	.47	.10	.60	.08
Dynamic skiametry vs. cross cylinder	.42	.11	.43	.11	.26	.13	.43	.11
Plus lens vs. cross cylinder	.31	.12	.37	.11	.46	.10	.42	.11

for the minimum and maximum of the monocular and binocular test. The formula for correlated measures was used in computing the S.D. of the difference. It will be seen that the differences are not very reliable, as the ratios of $\frac{D}{S.D._{diff.}}$ are all considerably less than three. This may be due to the fact that the number of cases used in the sample is small.

The Coefficients of Correlation. The coefficients of correlation between the negative accommodation tests as given in Table 6*a* indicate a very low degree of correlation between the various tests, the highest being .64 and the lowest, .26. This is exactly what one might expect when we consider the nature of the three tests. These are as follows: dynamic skiametry being an objective test, the plus lens, a subjective, and the Cross cylinder, a subjective test which causes a disturbance of the visual images due to the interval of sterum produced by the Cross cylinder lens.

General Summary

1. A series of tests was carried out on 26 subjects. Negative accommodation was tested, using three different types of tests for that function.

2. The minimum and maximum findings were obtained in each test. The right- and left-eye values were combined, thus giving 52 tests for the calculation of the average, range, and S.D. The coefficient of variation was computed from the average and the S.D. of all the subjects contained in the group.

3. Comparing the results obtained from testing negative accommodation, it is found that the dynamic retinoscopy has a coefficient of variation of 10.83 per cent, the plus lens test 21.51 per cent, and the Cross cylinder test 67.06 per cent for the monocular tests, and 11.51 per cent, 19.71 per cent, and 64.06 per cent, respectively, for the binocular tests.

4. Between the minimum and maximum dynamic retinoscopy for individual averages, the difference is between 0.25 D and 0.50 D.

5. It is also found that the monocular negative accommodation findings are consistently higher on all tests used. The amounts vary from 0.25 D to 0.50 D.

6. The reliability of all these tests was determined by correlating the odd and even tests and the coefficient of reliability was found to be .99+, with the exception of one or two instances where it was slightly lower.

7. The three different tests of negative accommodation were correlated and it was found that the coefficient of correlation was of very low degree for the negative accommodation tests.

Conclusions

1. From these results we may conclude that the minimum and maximum amounts of negative accommodation vary depending upon the tests used. Among the three types of tests used the dynamic retinoscopic test is the least variable, the plus lens and reading card test is more variable, and the Cross cylinder test is the most variable, i.e., about 600 per cent more variable than the dynamic retinoscopy test. We can, therefore, get less variable results by using the objective dynamic findings than through the use of any of the other subjective tests.

2. The Cross cylinder test, because of its very high coefficient of variability (77.83 per cent in the minimum and 56.30 per cent in the maximum monocularly, and 84.78 per cent and 43.35 per cent, respectively, binocularly), is not a proper test for measuring the function of negative accommodation and possibly does not measure this function at all.

3. The difference between minimum and maximum amounts of dynamic retinoscopy is between 0.25 D and 0.50 D.

4. The monocular negative accommodation is between 0.25 D to 0.50 D higher than the binocular. This is confirmed by all tests.

The Clinical Significance of These Tests

The results and conclusions reached in this study are of considerable clinical significance. While we find that all of the negative accommodation tests are very reliable, they differ greatly in their variability. Few practitioners today prescribe lenses from the findings of any single test. They do make use of the negative accommodation tests for the purpose of comparison; they are used as tools for diagnostic procedure. Now if we find that the Cross cylinder test is over 600 per cent more variable than the dynamic and over 300

per cent more variable than the plus lens test, we should be on our guard against attaching too great importance to this test, in spite of the fact that it enjoys great clinical popularity and has many advocates.

Any efficient skiascopist knows from clinical experience that, when a dynamic skiascopic test is performed and properly interpreted, it is possible to arrive at an accurate prescription for the reading point (providing there are no muscular imbalances) from this test alone.

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LA CONSTANCE ET LA VARIABILITÉ DES TESTS EMPLOYÉS POUR MESURER LA RÉPONSE ACCOMMODATIVE AU PROXIMUM

(Résumé)

Kurtz rapporte les résultats d'une expérience qu'il a faite pour déterminer la constance et la variabilité des tests employés cliniquement par les optométristes et les opthalmologistes pour mesurer la réponse accommodative au proximum, c'est-à-dire, la quantité d'accommodation qu'il faut exercer en employant les yeux au point de lecture, ce qu'on appelle dans l'optique physiologique NRA (c'est-à-dire, Accommodation Négative de Réserve).

Kurtz a employé trois tests différents, à savoir, (*a*) skiascopie dynamique, (*b*) plus test de lentille et (*c*) test cylindrique ("cross cylinder"), dont le premier est un test objectif, les deux derniers des tests subjectifs. On a constaté que les tests ont eu le coefficient suivant de Variation.

Test (*a*)	10,83 pour cent monoculairement	11,51 binoculairement
Test (*b*)	20,51 pour cent monoculairement	19,71 binoculairement
Test (*c*)	67,06 pour cent monoculairement	64,06 binoculairement

A cause du fait que le coefficient de variation du test (*c*) dépasse tant celui des tests (*a*) et (*b*), Kurtz met en question la validité du test (*c*) et maintient qu'il ne mesure pas la même fonction que mesurent les tests (*a*) et (*b*). Il trouve aussi que la quantité d'accommodation employée au proximum monoculairement est plus grande de 0,25 à 0,50 dioptères que celle employée binoculairement. Tous les trois tests ont confirmé ceci.

KURTZ

DIE ZUVERLÄSSIGKEIT UND VERÄNDERLICHKEIT DER TESTS, DIE ZUR MESSUNG DER ANPASSUNGSREAKTION AUF DEM NAHEPUNKT GEBRAUCHT WERDEN

(Referat)

Kurtz berichtet über die Ergebnisse eines von ihm ausgeführten Experiments, um die Zuverlässigkeit und Veränderlichkeit der Tests zu bestimmen, die klinisch von Augenärzten zur Messung der Anpassungsreaktion auf dem Nahepunkt gebraucht werden, d.h. die Menge der Anpassung, die nötig ist, wen man liest. Dies wird in der physiologischen Optik als NRA (d.h. negative Reservenanpassung) bezeichnet.

Kurtz gebrauchte drei verschiedene Tests, nämlich (*a*) dynamische Skiaskopie, (*b*) Plus-Linsentest, und (*c*) Kreuzzylindertest. Der erste ist

ein objektiver Test, die zwei letzten sind subjektiven Tests. Die Tests hatten den folgenden Koeffizienten der Veränderlichkeit.

Test	(*a*)	10,83 Prozent	einäugig	11,51 zweiäugig
Test	(*b*)	20,51 Prozent	einäugig	19,71 zweiäugig
Test	(*c*)	67,06 Prozent	einäugig	64,06 zweiäugig

Angesichts der Tatsache, dass der Koeffizient der Veränderlichkeit des Tests (*c*) so viel grösser als der von Tests (*a*) oder (*b*) ist, stellt Kurtz die Gültigkeit des Tests (c) in Frage und behauptet, dass er dieselbe Funktion nicht misst, die Tests (*a*) und (*b*) messen. Er hat auch festgestellt, dass die Menge der Anpassung, die auf dem Nahepunkt einäugig gebraucht wird, zwischen 0,25 bis 0,50 Dioptern höher liegt als die, die zweiäugig gebraucht wird. Dies wurde durch alle drei Tests bestätigt.

KURTZ

TWO ADDITIONAL ATTEMPTS TO CONDITION THE PUPILLARY REFLEX*

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In a previous paper (3) an attempt was made to repeat the experiments of Hudgins (1) on the conditioning of the pupillary reflex. The report of this investigation (3) drew rather sharp criticism from Dr. Hudgins (2), who asserted that:

> 1. His experiment was *not* repeated because the number of trials given during any training step in our experiment was limited by the number given by him. Since, therefore, each succeeding step was not based on an already present, firmly established conditioned response, Dr. Hudgins holds our study to be "less than an adequate check" (2, p. 208) on his own findings.
>
> 2. The fixation point employed in our apparatus was so close to the eye that marked accommodation and convergence responses would ensue which would so constrict the pupil that further conditioned contraction could not be expected.

The conditions under which pupillary conditioning may be demonstrated can be settled only by experiment, not by debate. We propose, therefore, to let the results of two additional experiments supply our answer to Hudgins' (2) criticisms.

EXPERIMENT I

In an attempt definitely to establish a conditioned pupillary response, four subjects were trained with our original apparatus (3). Step I (1, 3) was omitted because the use of a buzzer or bell in the training sequence is solely a matter of precedent. For this reason, the conditioning stimuli were hand squeeze and the verbal commands "contract" and "relax," following the procedure of Steps II and III (1, 3). That is, at the experimenter's command, "con-

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[1]The writer is greatly indebted to Professor Samuel Renshaw under whose advice and direction these experiments were made.

tract," the subject would squeeze the dynamometer[2] handle. This would close the light circuit. The training light (37.66 apparent meter candles) would then appear. At the command, "relax" (given four seconds later), the subject would release his grip. The light circuit was then broken and the eye remained in darkness for ten seconds, after which time the trial was repeated.

Two of these subjects (Group A) operated the dynamometer switch with the left hand only. The other two (Group B) were instructed to contract the right hand simultaneously with the left, on the theory that additional reinforcement of the decussating fibres might ensue and hence make for more rapid conditioning.

All subjects received 1100 paired stimulations extending over a period of 12 days. One hundred trials were given daily (Sunday excepted) with rest periods at the end of each 25. Tests were made at the termination of each sitting. After 600 trials the subjects were requested to repeat the verbal commands subvocally (Step III).

Three of the subjects gave no clear-cut conditioned responses to the hand squeeze or to the verbal commands as given by either the experimenter or the subject himself.

The other subject (Group B) showed some evidence of conditioning. Responses similar to that portrayed in Figure 3, Test 2 were elicited by the verbal commands. Attempts to obtain the response subsequent to subvocalization on the part of this subject failed. The response also could not be evoked after a 24-hour period of no training. One hundred more trials were then given this subject (1200 total). The results were the same as on the previous day. Such conditioned responses as were obtained were extinguished by four to six tests.

In his discussion (2), Dr. Hudgins suggests that in our apparatus (3) the fixation distance of six inches, even with the three-diopter spherical-convex lens in position, so disturbed the ocular mechanisms that unambiguous conditioned responses were impossible.

A group of five subjects, highly trained in psychological investigation, was used to check this contingency. These subjects were requested to fixate the cross hairs at a distance of six inches with

[2]The Smedley Dynamometer (3) was replaced by a pincer-like instrument that, while evoking a definite contraction of the fingers, operated easily, thereby eliminating possible inhibitory factors elicited by cramping of the hand.

the lens in position and at a distance of 16 inches (approximately 40 cm.) without the lens. (We shall present evidence to show that in Hudgins' apparatus the actual fixation distance is 40 cm.) The pupil was observed under each condition over an interval of approximately one minute. Illumination remained constant. During these two periods no differences in pupillary size or behavior could be distinguished.

Experiment II

Following these experiments, a new apparatus was built that was as precise a duplication of Hudgins' (1) instrument as could be constructed.[3]

Preliminary experimentation with this piece indicated the possibility of improvement in the method of obtaining a test light. Hudgins (1) reduced the training light by decreasing the opening in the iris diaphragm. This also could be done with the instrument used here. It was discovered, however, that a narrow beam of light tangentially impinging upon the pupil made for a more highly illuminated cornea with no apparent decrease in pupillary size. This beam issued from double metal tubes (one sliding over the other) inclosing a 21-c.p. automobile headlight bulb operated on six volts D.C. The centers of the solid ends of these tubes were pierced with 3/64-inch holes and served as baffle plates for the light rays. The tubes were held by an adjustable bracket in a position four and one-half inches from the temporal side and slightly posterior to the right eye. This instrument emitted an only slightly diverging beam that, striking the outside of the cornea, spread evenly over the entire surface, completely illuminating the iris but permitting but little light actually to enter the pupil, and, because the beam was almost parallelized, very little stray light entered the sclera. No difference in pupillary diameter could be distinguished, regardless of whether the eye was illuminated by the tangential beam or by the usual test light (3.42 apparent meter candles) (1, 3). Since the boundary of the pupil was so markedly more visible under the former condition, this method of test light illumination was used here. Records of

[3]Dr. Hudgins has carefully examined this apparatus and pronounced himself satisfied that it is a true reproduction of his own. He has also seen records from subjects trained with this instrument and accepted them as valid.

pupillary movement made under both conditions subsequent to training showed no distinguishing characteristics peculiar to either method.

This method of illumination, we feel, represents a rather important improvement in technique for all experiments in which the eye is to be kept partially dark-adapted and yet made sufficiently visible for examination or photographing.

With the eye in position and the tangential beam turned on, there was no light directly entering the pupil. A fixation object was provided by the reflection of the cornea on the face of the prism, a distance of 40 cm.

Although Hudgins (1, 2) states the fixation point to be 136 cm. from the eye [the distance from the eye to the center of the prism (40 cm.) plus the distance from this point to the light source (96 cm.)], it would seem that the actual fixation distance would be 40 cm. (from the eye to the prism). Certainly this is where the cross-hairs are subjectively *localized* by the observer. (Naïve subjects, when asked to do so, invariably located the light source at a point in space about 16-18 inches before the eyes.) There is still, however, additional evidence.

The training light (37.45 apparent meter candles) established rather enduring after-images. If the subject, during a rest period, attempted to "project" this after-image onto a distant surface, it was found that Emmert's law was not fulfilled. The after-image remained at a point in space approximately 16 inches before the eyes. Apparently it is virtually at this distance where the cross-hairs are seen. (The failure of the after-image to follow Emmert's law may be due to the total lack of any reference surface in the emptiness out of which the light appears in this apparatus.)

With this apparatus also, pupillary movements were followed by the lever arrangement reported before (3). The amplification was such that 1 mm. on the eye gave 4.3 mm. on the polygraph tape.

Six subjects were trained with this apparatus. Four were entirely familiar with the problem; two were naïve.

The entire investigation (1, 3) was to be repeated. Each step, if possible, was to be based on a firmly established conditioned response.

On the first day the subject was seated before the apparatus, instructions were given him, and the initial tests were made, following the procedure previously reported (3). The next day the subject

returned and was given 100 paired stimulations of buzzer and light. Rest periods were given at the end of each 25 trials. No tests were made at the end of this sitting. The third day another 100 trials were given, following which tests were made for the presence of conditioning. In testing, the tangential beam was turned on, and, when the pupil reached a relatively steady state, the buzzer was sounded. After the buzzer ceased, the pupil was observed over periods ranging from 20 to 100 seconds. A rest period ordinarily was then given before the next test. Sometimes two or three tests would be made with no intervening rest. The results were, in so far as could be determined, identical with those obtained with our original apparatus (3). Training with the buzzer was then continued.

To avoid the danger of extinction because of too great reinforcement, occasional tests would sometimes be made subsequent to the rest periods. At other times the tangential beam would remain on throughout the training period and tests would be inserted into the regular trial sequence. Regardless of how or when tests were made, the results remained negligible.

TABLE 1
A Summary of Results of Tests for the Presence of the Conditioned Iridic Buzzer

Subject	Paired stimulations	Number of tests	Positive indication of *CR* to buzzer
A	300	3	2
B	300	2	0
C	300	3	0
D	300	3	1
E	300	2	0
F	300	3	0
Total		16	3
Percentage of all tests indicating conditioning = 19			
A	600	3	1
B	600	3	0
C	600	3	0
D	600	3	1
E	600	3	1
*F**			
Total		15	3
Percentage of all tests indicating conditioning = 20			

*Due to unforseen circumstances it was impossible to continue training with this subject beyond 300 trials.

Table 1 shows the results obtained subsequent to 300 and 600 trials. Test 2 in Figure 3 is a typical sample of a "positive" indication. As may be seen, such responses were the exception rather than the rule. They appeared in practically "random" order. There was no indication of regularity or permanence. After 300 paired stimulations, 19 per cent and, after 600 trials, 20 per cent of all tests gave evidence for conditioning. We have surely the right to expect that more than 20 per cent of all tests should show the presence of conditioning if the principle is to serve as an adequate description of the genesis of voluntary behavior.

A typical record is shown in Figure 1. These observations were

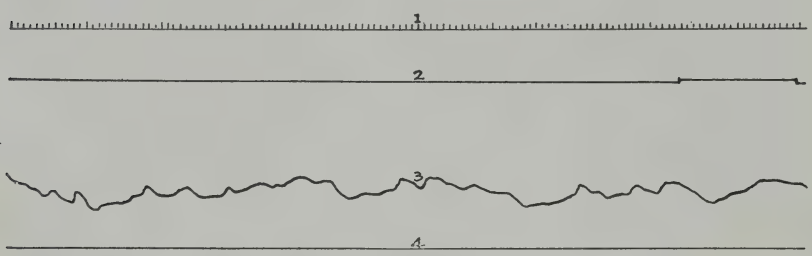

FIGURE 1

RECORDS READ FROM RIGHT TO LEFT

1. Time in seconds
2. Buzzer line
3. Tracing of pupillary movements
4. Base line

made following the 600th paired stimulation of buzzer and light.

This record shows again the extreme difficulty in attributing pupillary movement only to the conditioning effect of the buzzer. If the initial contraction (movement *toward* the base line) were greater in amplitude than those subsequent to the test, one would feel perhaps justified in considering it a true conditioned response.

Further evidence for the ambiguity of these movements is found in Figure 2, taken from another subject. This was made at the beginning of the test period (after 600 trials) before the buzzer had been sounded. A rather prolonged contraction having the general characteristics of the conditioned responses reported by Hudgins (1) is indicated between the two arrows. Since this sort of phenomenon was rather common for all subjects, we could never

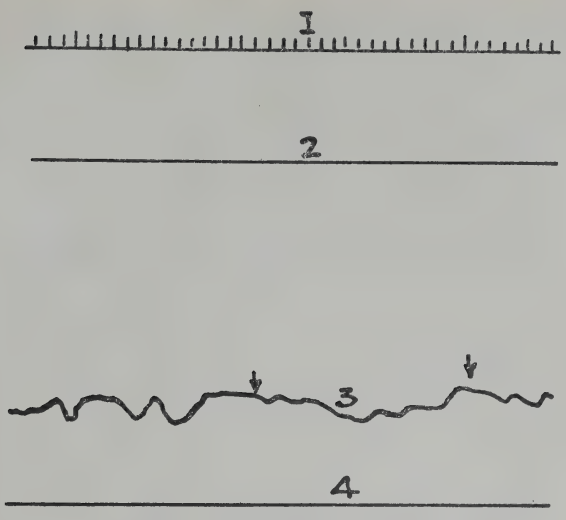

FIGURE 2

RECORDS READ FROM RIGHT TO LEFT

1. Time in seconds
2. Buzzer line
3. Tracing of pupillary movements
4. Base line

be certain that those responses apparently aroused by the buzzer would not have occurred in the absence of any stimulation. That they take place both before and after testing these records will abundantly illustrate.

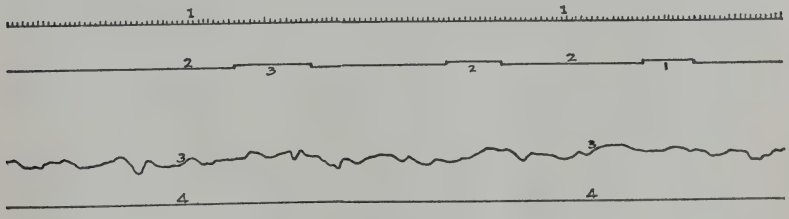

FIGURE 3

RECORDS READ FROM RIGHT TO LEFT

1. Time in seconds
2. Buzzer line
3. Tracing of pupillary movements
4. Base line

Figure 3 shows the results obtained during three tests (indicated by breaks in the "buzzer" line) that followed the event described above. With the possible exception of Test 2, there is no conspicuous instance of conditioning. In the light of Figure 2 any possible interpretation of Figure 3 becomes yet more hazardous. As may be seen, whatever connotation might be inherent in Test 2 is not apparent in Tests 1 and 3.

Since, after 600 paired stimulations, the obtained data were such as these, there seemed to be no reason for continuing on to Steps II and III.

Hudgins' experiment has been repeated with two pieces of apparatus, one identical with his own, the other like his in principle. The data obtained from both instruments have been consistent. In neither case have conditioned iridic responses clearly evidenced themselves. That our results diverge so markedly from those of Hudgins can be due only to some factor or factors uncontrolled by him or by ourselves. We have most scrupulously followed his procedure and method in our attempts to establish unambiguous conditioned pupillary responses. To date, we have been unable to do so.

The results of this study confirm those previously reported (3). There is evidence that our failure to obtain clear-cut conditioned iridic responses does not lie in any defect in our apparatus or technique. We have again been unable to demonstrate that the conditioned pupillary reflex fulfills the demands of an adequate description of the genesis of voluntary behavior.

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DEUX AUTRES ESSAIS DE CONDITIONNER LE RÉFLEXE PUPILLAIRE

(Résumé)

Notre essai antérieur (ce Journal, 1934, **11**, 3-23) de répéter les expériences de Hudgins (ce Journal, 1933, **8**, 3-51) a fait venir des critiques assez sévères de lui (ce Journal, 1935, **12**, 208-214). Pour répondre à ses objections, on a fait deux autres expériences.

En employant notre premier appareil, on a entraîné quatre sujets avec des stimulations en paires de commandes verbales et de lumière. On a donné jusqu'à 1100 épreuves avec des tests intermédiaires. On n'a obtenu aucuns résultats signifiants.

On a construit ensuite une duplication précise de l'appareil de Hudgins. On a entraîné six sujets avec la sonnette bourdonnante et la lumière combinées. On a fait subir 600 épreuves. On a obtenu des réponses conditionnelles positives dans environ 20 pour cent de tous les tests. On considère que de tels témoignages ne sont pas satisfaisants pour une description de la genèse du comportement volontaire.

STECKLE

ZWEI WEITERE VERSUCHE ZUR BEDINGUNG DES PUPILLEN REFLEXES

(Referat)

Unser vorheriger Versuch (diese Zeitschrift 1934, **11**, 3-25), Hudgins Experimente (diese Zeitschrift 1933, **8**, 3-51) zu wiederholen, erhielt eine ziemlich scharfe Kritik von ihm (diese Zeitschrift 1935, **12**, 208-214). In Erwiderung auf seine Einwände sind noch zwei Experimente ausgeführt worden.

Wir haben unseren vorherigen Apparat angewandt und vier Vpn. wurden mit gepaarten Reizen von wörtlichen Befehlen und Licht eingeübt. Bis zu 1100 Versuchen wurden mit dazwischenliegenden Tests gegeben. Keine bedeutsamen Ergebnisse wurden erhalten.

Eine genaue Nachbildung des Hudgins Apparats wurde dann gebaut. Sechs Vpn. wurden dann mit einer Verbindung von Laut und Licht eingeübt. 600 Versuche wurden gegeben. Positive bedingte Antworten wurden in etwa 20 Prozent aller Tests erhalten. Solche Evidenz wird als nicht hinreichend zur Beschreibung der Entstehung des willkürlichen Verhaltens angesehen.

STECKLE

SOME CHARACTERISTICS OF PARAFOVEAL STIMULI AND THEIR AFTER-IMAGES*[1]

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O. D. KNIGHT

INTRODUCTION TO THE PROBLEM

The visual after-image has been a phenomenon of systematic importance at least since the time of Aristotle; it has figured as a bone of contention since the joining of battle between the advocates of the Young-Helmholtz theory of color-vision and the followers of Hering, and its status still remains undecided. From certain observations of its behavior, some of which are thought not to have been described before, an attempt is made in this paper to clarify its theoretical position. Having been presented so frequently as a supporting argument in favor of one explanation of color-vision or another, it has perhaps been over-conventionalized, and has suffered from a lack of experimental investigation making it the primary object of interest. The earlier observations of Purkinje, Goethe, Fechner, Aubert, and others were pressed into service by v. Helmholtz and Hering, each emphasizing some aspects of positive and negative after-images and passing lightly over others, as the best interests of his theory required. Hering, borrowing from Plateau, postulated an active, autonomous process, opposite in character to the process of excitation or allonomous change, as the basis of the negative after-image; v. Helmholtz, following Goethe and Fechner, considered the passive condition of the retina after stimulation as its basis. The positive after-image offers difficulties to both theories. Our only present interest in the debate is to recognize that from it a stereotyped and partly erroneous conception of the after-image, common to both sides, has come to be widely held. This, briefly, is that the excitatory process continues in the photoreceptors for a certain length of time after the termination of a light stimulus, producing a positive after-image like the

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[1]The author wishes to express his gratitude for the suggestions and help he received from Professor Samuel Renshaw.

stimulus-object in hue and in contrasts of light and dark areas; next there is recovery from the fatigue of excitation, and, since in patterned vision there is unequal fatigue or adaptation of different retinal areas, negative after-images result, of complementary hue to that of the stimulus-object, and reversed in intensity-values.[2] The complete fading of the negative after-image denotes complete retinal recovery; since it diminishes in size and its edges grow hazy, the recovery process is somewhat slower in the middle of the image.

A number of demonstrations of the inadequacy of this conventional scheme, to which the present report is supplementary, have been made. Katz (1) has explicitly criticized the retinal-adaptation assumption, and has adduced a variety of evidence against it. Jaensch (2) in particular has depicted an extended continuity between the most immediate after-effects of optic stimulation and the persisting memory-trace, including in one connected series all the stages of after-images, the eidetic image, and the memory image. As an example of how unsatisfactory a basis for the explanation of after-images local retinal conditions afford we may cite Jaensch's finding that a visual pattern of a certain intensity will produce a positive after-image if it is of interest to the observer, negative if it is not. McDougall (3) has shown that borders graduated from the brightness of the figure to the darkness of the ground constitute in themselves a condition favorable for the production of positive after-images; also that the duration of both positive and negative phases is about the same under given conditions—that is, the longer the positive lasts, the shorter will be the duration of the negative. He has demonstrated the production of a positive after-image of unusually long duration from fairly intense stimulations of the dark-adapted eye with durations of stimulation subliminal for the production of a negative after-image. With slight variations the same phenomenon has been studied by Swindle (4) and Dimmick (5). The latter found that no after-image of a red parafoveal stimulus nor of a white central stimulus of less than foveal size was obtained.

Rexroad (6), observing that drifts were set up in the negative after-images from a large parafoveal stimulus after fixation of a small, less intense stimulus at its side, concludes that, because eye-

[2]In this connection, it should be noted that Wright (12) has rejected both his own and Hecht's theories of intensity discrimination based on the photochemical adaptation process in the retina.

movements incompatible with the drift destroyed the after-image, it must consist only of tensions in the extrinsic ocular musculature; if little or no drift is observed, balancing tensions have been set up. Walters and Gundlach (7), repeating Rexroad's experiment, failed to verify his results, and object to his theoretical conclusions on the ground that qualitative differences and spatial patterns of after-images vary independently of any specific pattern of muscle-tensions. They conclude that the after-image is dependent on the local chemical after-effects of retinal stimulation, "as most theories of vision hold."

Swindle (4) asserts that a positive after-image stage normally precedes the negative if careful fixation has been maintained both during impression and subsequent to impression. Thus a loud sound, occurring at the instant of termination of the stimulus makes it impossible to see this primary positive stage, because, according to Swindle, it induces reflex movements of the eyes.

Apparently little work has been done on the modification of the form properties of the negative after-image except in relation to the characteristics of the ground on which it is "projected." Shuey (8) notes incidentally that an intermediate diamond-shaped stage occurred between an initial Greek cross and a final formless blur, when the stimulus was a Greek cross. Rothschild (9) found that the negative after-image undergoes simplifying and normalizing tendencies; if irregularities in the stimulus-figure are not so pronounced as to be prominent in their own right, they are reduced or absent in the after-image. On the other hand, both Katz (1) and Braddock (10) deny that the negative after-image in a strict sense has form.

A favorite point of attack on the fatigue assumption has been the appearance of a flight of colors in the after-imagery from white light, whose after-effects should be achromatic. McDougall (3) describes a similar flight when the stimulus is chromatic light of any hue; here the negative after-image should be complementary. Hartmann (11) has shown that the negative after-image of an ambiguous figure with differently colored halves changes from one blended color to two separate complementary colors as the perception of the spatial pattern shifts from a single to a double meaning.

If sufficient evidence of this sort can be brought against the concept of the after-image as a retinal fatigue or adaptation phenomenon, the question of how it shall be accounted for is left open. It is the

purpose of this study to point the question and to suggest a tentative answer.

Problem

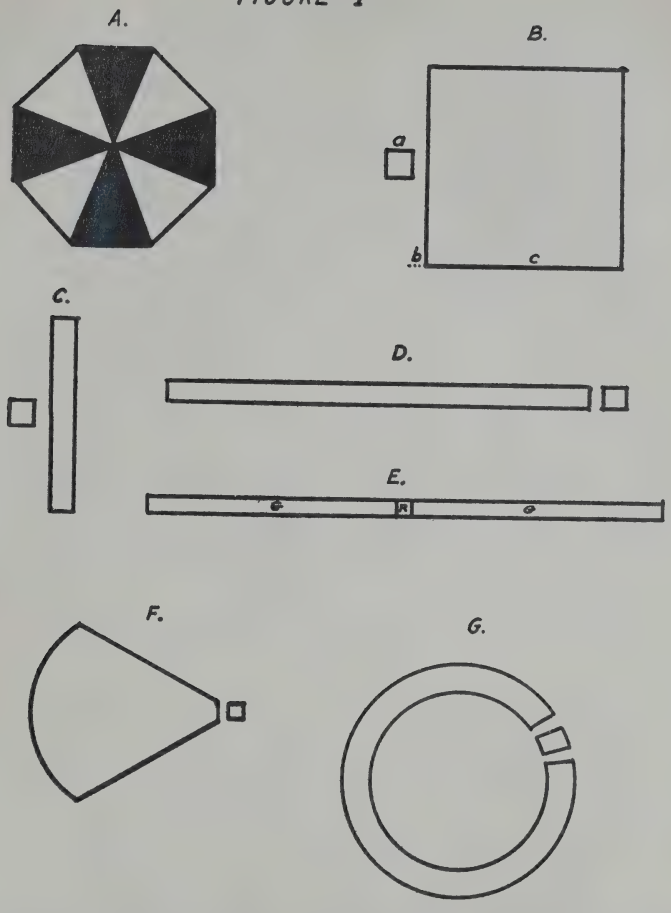
The object of the present experiment is to describe—rather than to analyze quantitatively—certain after-image phenomena which do not lend themselves readily to the fatigue-assumption, nor to the general concept that after-images are solely the result of local photochemical processes in the retinal elements. In particular the relation of parafoveal to foveal stimuli and after-images has been of interest.

Procedure

The observer sat in a dark room before a vertical screen, 59 x 71 cm., of matt black cardboard, at a distance of 150 cm. From the other side a 90-watt focussing projection lamp threw a beam on the center of the screen, where a circular window of 175-mm. diameter was cut. To each side of this window, and below it, on the back of the screen, flat tabs were pasted so that circular black cards of the same cardboard, and of 180-mm. diameter, could be slipped in; these fitted snugly against the screen, and permitted no light to filter through around the outside of the window. In the circular cards the stimulus figures were cut as openings over which, on the back, various papers were pasted. For the most part single thicknesses of standard Hering papers were used; these when illuminated furnished luminous patches in good contrast to the surrounding field. For chromatic patches only the red No. 1, yellow No. 5, green No. 9, and blue No. 13 of the standard Hering papers were used; these frequently provided a useful means of identifying the after-images. The beam of the lamp was so focussed as to include only a little more area on the back of the card than that occupied by the figure to be presented. The standard duration for impression was 20 seconds.

When alternating colors in the stimulus figures were required, a disc with sectors of colored paper mounted on the shaft of a variable-speed pulley which in turn was belt-driven by an electric motor was placed behind the screen, with the desired figures cut in the card as open windows. Six observers sat for the entire series of stimulus-figures. One was a professor of experimental psychology; five were graduate students in psychology. All were well trained in making

FIGURE 1

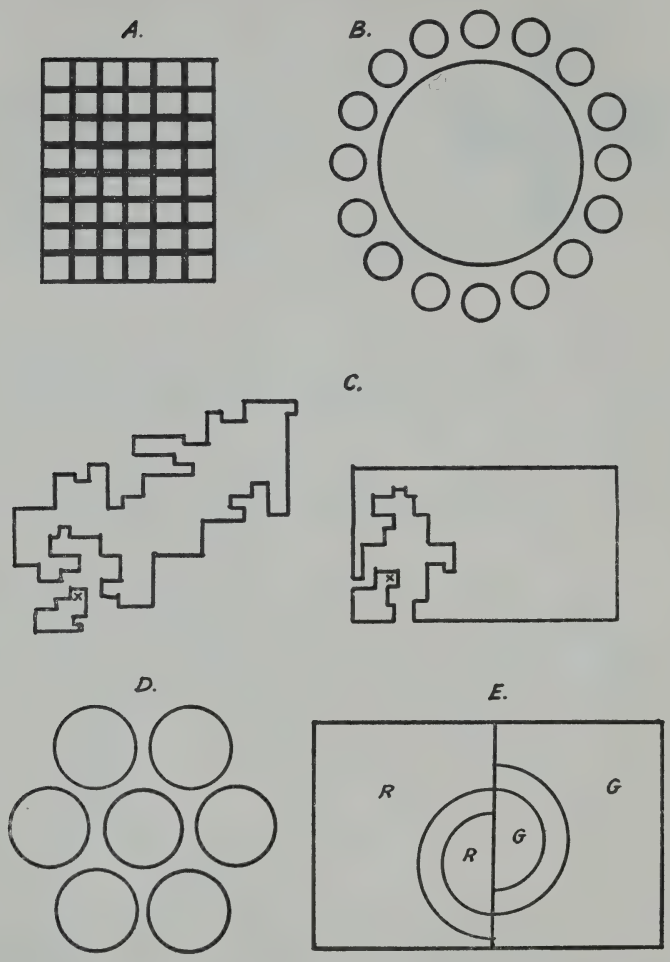


observations of after-images. For the repetition of Rexroad's experiment (6) 17 additional observers were used; these were graduate and undergraduate students in psychology, and most of them were naïve. Details of the specific stimulus-figures used are given in conjunction with observations below.

Results

Size and Intensity. The standard figure is Figure 1, *B*. Side *a* of the small square is five millimeters; distance *b* is three

FIGURE 2



millimeters; side *c* of the large square is four centimeters. Twenty-three observers were in substantial agreement on the visual effects. Postponing drift effects described by Rexroad until later, the principal effect observed in the figure, which duplicates Rexroad's stimulus figure when the small square is made less intense than the large, is that there is no after-image of the small square after it has been fixated, but that it does produce an after-image if fixation has been

on a point to one side of it, either in the large square or not. Details of variations of size and intensity of these squares and the resulting visual after-effects follow.

Foveal and Parafoveal After-Images

(*fAI* and *pfAI*)

Standard observation distance: 150 cm.
Standard square: 3 x 3 mm., 9 sq. mm.
Standard separation of squares: 7 mm.

Brilliance values: *B* (Hering black paper, giving a dark grey stimulus patch) gives .000017 foot-candles per sq. mm. at the observation distance. This is called one unit. *G* (Hering medium gray paper) equals 9*B* units. *W* (a thin white tissue) equals 108*B* units.

The left (*L*) square is kept standard; the right (*R*) varies in size.

Case 1

L and *R* standard; *L* 81 units (*G*); *R* 81 units (*G*)
Size 1 : 1; Illumination 1 : 1

L fAI: faint, persistent negative
L pfAI: distinct negative, persists

Case 2

L standard, 81 units (*G*); *R* standard, 972 units (*W*)
Size 1 : 1; Illumination 1 : 12

L fAI: faint negative, appears late, short-lived
L pfAI: stronger than *Lf*, weaker than *Rpf*, not late

R fAI: weaker than *Rpf*, late, short-lived negative
R pfAI: distinct negative, not delayed

Case 3

L standard, 81 units (*G*); *R* 6 x 6 mm., 3888 units (*W*)
Size 1 : 4; Illumination 1 : 45

L fAI: extremely faint, short-lived, late negative
L pfAI: late, short-lived, but fairly distinct negative

R fAI: early, faint positive, quickly becomes negative
R pfAI: strong, persistent negative

Case 4

L standard, 81 units (*G*); *R* 6 x 6 mm., 324 units (*G*)
Size 1 : 4; Illumination 1 : 4

L fAI: faint, late, short-lived negative
L pfAI: early, distinct, persistent negative

R fAI: faint, late, short-lived negative
R pfAI: early, distinct, persistent negative

CASE 5

L standard, 81 units (*G*); *R* 9 x 9 mm., 729 units (*G*)
Size 1 : 9; Illumination 1 : 9

L fAI: only an extremely faint trace of a negative; appears very late
L pfAI: early, distinct negative; fades before *R fAI*

R fAI: fairly distinct negative, but late
R pfAI: early, persistent, distinct negative

CASE 6

L standard, 972 units (*W*); *R* 6 x 6 mm., 324 units (*G*)
Size 1 : 4; Illumination 3 : 1

L fAI: faint positive, winks, becomes persistent negative
L pfAI: faint positive, becomes negative more quickly than *L fAI*

R fAI: does not appear until *L pf* becomes negative, then appears as a faint negative
R pfAI: distinct negative, persistent, but a little delayed in appearing initially

CASE 7

L standard, 81 units (*G*); *R* 9 x 9 mm., 11,666 units (*W*)
Size 1 : 9; Illumination 1 : 130

L fAI: none
L pfAI: delayed; appears only when *R fAI* becomes negative; then appears as a fairly distinct negative

R fAI: long blur, then positive, quickly becoming distinct, persistent negative
R pfAI: early positive, winks, becomes negative

CASE 8

L standard, 972 units (*W*); *R* 9 x 9 mm., 729 units (*G*)
Size 1 : 9; Illumination 9 : 7

L fAI: faint, very late negative
L pfAI: begins as a faint positive image, becomes negative

R fAI: does not appear until *L pf* becomes negative, then appears as a weak negative, but fairly persistent
R pfAI: early, distinct, persistent negative

CASE 9

L standard, 81 units (*G*); *R* 15 x 15 mm., 2025 units (*G*)
Size 1 : 25; Illumination 1 : 25

L fAI: none
L pfAI: early, distinct, fairly persistent negative, but fades before *R fAI*

R fAI: barest trace of faint negative *AI;* short-lived, late
R pfAI: early, distinct, persistent negative

CASE 10

L standard, 972 units (*W*); *R* 15 x 15 mm., 2025 units (*G*)
Size 1 : 25; Illumination 1 : 2

L fAI: none
L pfAI: early, faint positive, short-lived, not succeeded by a negative

R fAI: negative, distinct, but a little late
R pfAI: early, distinct, persistent negative

CASE 11

L standard, 972 units (*W*); *R* 21 x 21 mm., 3969 units (*G*)
Size 1 : 49; Illumination 1 : 4

L fAI: none
L pfAI: faint positive, becomes negative, persistent

R fAI: distinct, persistent negative, but late
R pfAI: early, distinct, persistent negative

CASE 12

L standard, 9 units, (*B*); *R* 21 x 21 mm., 31,752 units (*W*)
Size 1 : 49; Illumination 1 : 3526

L fAI: none
L pfAI: long dazzle, then faint negative, short-lived

R fAI: dazzle, then persistent distinct negative
R pfAI: dazzle, persistent distinct negative

In all cases with *L* standard size and *R* 21 x 21 mm. or larger, the *L fAI* is always absent, no matter what intensity combination is used. For example, *L W* and *R B* does not produce a *L fAI,* in spite of the intensity of the *L* stimulus, and in spite of the fact that the same *L* alone does produce an after-image; even, in the case of smaller parafoveal stimuli (as Case 6 above), a positive after-image. Conversely, for these sizes of *R* larger than those given in the twelve

cases above, a *L pfAI* is always present, with any combination of intensities of *B, G,* and *W*. Thus, *L B* and *R W* does not prevent the appearance of a *L pfAI,* in spite of the relatively small size and low intensity of the former. From these facts it appears that intensity is not as cogent a factor as size of stimulus-patch.

It is necessary to specify the exact meaning of foveal stimulus. Strictly speaking, it refers to a centrally fixated light-patch subtending a visual angle of one degree or less. In the cases above it has been used more loosely to signify a homogeneous patch—regardless of size—on some point in which fixation is maintained. In the latter case, if the size materially exceeds one degree of visual angle, it is plain that both foveal and parafoveal areas are being stimulated simultaneously, and that the stimulus is single only in the sense of its apparent homogeneity.

From the reports of all observers, certain effects are clearly established. A parafoveal light stimulus is quicker in development, more "sensitive," briefer in the course of consecutive stages through which it runs than a foveal stimulus of like size, color, and intensity. In impression, it gives an immediate effect of greater brilliance, but a much earlier decrement in saturation—if a colored stimulus is used—and in intensity. A red or green (Hering No. 1 and Hering No. 9) parafoveal stimulus, for example, begins as filmy, textureless, hazy-contoured, rich in chroma, and raying (surrounded by a luminous haze of like hue), but desaturates much more quickly than a foveal stimulus, becoming yellow, then whitish, as if it were moved toward the extreme periphery in perimetry. Then for a short space it blinks, or briefly disappears. This disappearance is considerably lengthened if a relatively still fixation of the foveal stimulus can be maintained, but even under the best conditions is transient. The halo about the vanished patch remains visible. Eye-movements tend to restore the initial appearance of the parafoveal stimulus. Under favorable conditions a fairly regular rhythm of such disappearances and reappearances takes place. The duration of the parafoveal stimulus between disappearances is directly proportional to its area. Intensity up to the value that produces a positive afterimage of more than momentary duration does not alter the effect. The duration of the absence of the parafoveal stimulus is inversely proportional to its area. If foveal and parafoveal stimuli are equal in size, and of alternating red and green of approximately equal

intensity, the parafoveal stimulus blends into yellow at a much lower rate of alternation than the foveal, and tends to disappear, as before, except that, instead of being a consistent absence, there is now a pronounced flicker produced by alternation of the absence of the stimulus and its presence in the yellow secondary stage. The rate of this flicker is observably different when on the one hand foveal and parafoveal stimuli become red or green at the same time and when on the other the foveal stimulus becomes red as the parafoveal becomes green, and vice versa. In the former case, there is a subjective impression that the flicker is definitely related to the speed of rotation of the colored sectors. In the latter the flicker seems faster.

If a parafoveal patch is surrounded by a foveal area considerably greater, as red surrounded by green, the parafoveal area does not blink out as it does when it is surrounded by a dark field, but passes through the following three stages: rich, filmy red, with a red halo, yellow, and green with a yellowish glow. As before, these effects are transient, and are destroyed by eye-movements. Initially there is simultaneous induction of color: the red parafoveal patch becomes more saturated; a gray one becomes reddish, etc.

In definiteness and contrast with its background the negative after-image from a parafoveal stimulus, expecially in early stages, equals or exceeds that from a comparable foveal stimulus. The size of the parafoveal stimulus at the standard observation distance of 150 centimeters may be as little as two millimeters square, or about five minutes of visual angle. For smaller sizes it is practically impossible to distinguish negative after-images from entoptic effects. The intensity, even for the smallest observable sizes, may be as little as one hundred thousandth of that of the foveal stimulus, and absolutely as little as .00015 foot-candles. The propinquity of a brighter foveal stimulus does not weaken or retard the parafoveal after-image appreciably, even when the foveal stimulus is a large ground completely surrounding the parafoveal, with parts equally distant and parts even more distant from the fixation point than the parafoveal stimulus.

A parafoveal after-image from any stimulus of sufficient area and intensity to yield an after-image with no foveal stimulus present will also yield an after-image with a foveal stimulus present in any size and intensity within the limits of observations made in this study.

On the other hand, a foveal stimulus which alone will yield a negative after-image may yield none, and always a weakened one, if a parafoveal stimulus is present in impression. In general the area of the parafoveal stimulus seems to be more significant in exerting a weakening effect on the foveal after-image than its intensity.

Movement. When foveal and parafoveal stimuli are alternately red and green, both appear to pulsate, the red constituting a nearer and larger phase, the green a smaller and more distant one. Thus an illusion of movement, the "gamma effect," is observed. When the two stimuli are steady (do not alternate), there is, regardless of color, a visible pulsation if careful fixation is maintained. Both seem to swell and shrink in a certain rhythm, which is not that of the heart-beat. The effect is more pronounced in the parafoveal stimulus.

The tendency, reported by Rexroad (6), for the after-image to drift in the direction of a brighter and larger parafoveal stimulus, was found to hold true only in the case of upward drift. No individual was found for whom more than one of the other three directions, right, left, and down, held unequivocally. There were considerable variations among individuals, but each individual showed the same idiosyncrasy upon repetition of the experiment. The amount and velocity of the drift became noticeably less with practice. The course of the actual movements observed seemed to result from a competition between the direction instigated by the stimulus squares and the individual's permanent tendency, in which the former gradually gave place to the latter. After-image drift was faster, less jerky, subject to fewer changes of direction, and more subject to voluntary control than the autokinetic streaming phenomenon. In contrast to Rexroad's statement, and confirming the findings of Walters and Gundlach, all observers reported that they could voluntarily modify to greater or less extent the drift of the after-image. This was accomplished by movements of the head and/or eyes. The periodic fadings, disappearances, and recurrences already referred to could not be prevented by any plan of eye movement. No pulsation was observed in any type of after-image.

Form. If the larger square in the standard Rexroad set-up (Figure 1, *B*) is reduced to a relatively narrow vertical bar (Figure 1, *C*), the after-image of the small square as a foveal stimulus is not absent, but appears faint and late as a negative after-image.

This indicates that the spatial spread of the adjacent parafoveal stimulus does not exert its full weakening effect, and that a certain area must be excited before the after-image of the small square will be entirely missing. Area, however, must be in a certain fairly compact mass. A standard small square at the end of a long, relatively narrow bar (as Figure 1, *D*), although adjacent to a parafoveal stimulus of approximately equal area to that of the larger square in Figure 1, *B*, is only partially weakened in the after-image phase; but here an entirely novel effect comes in; as the negative after-images persist, that of the bar after a time flows over and occupies the space of the smaller figure and of the intervening gap, so that only one bar of homogeneous color results. This effect is fluctuating, but recurs as long as the after-image can be made out. If, for example, the red square in Figure 1, *E* is fixated, the course of the after-image is as follows: a solid purple bar, two purple arms with an unfilled gap between them, and then the two purple arms with a faint green square between; later there are more or less irregular recurrences of any or all of these stages. If, on the other hand, a point in one of the green arms a few millimeters to the side of the red square is fixated, the green after-image of the red square is most prominent at the beginning, but fades early, leaving a gap across which the two purple bars flow, filling out a single long negative after-image. As before there are irregular recurrences.

If the horizontal bar is made all green and a point in the middle is fixated, a single purple after-image results, but this fades and breaks in two in the middle, after which the two remaining bars fluctuate in length by gaining and losing on their inner—never on their outer—ends. They may meet again in the middle several times, but tend toward small, widely separated pieces at the last. During impression the ends seem more brilliant than the fixated middle, and are surrounded by elliptical hazes resembling the "lines of force" around the ends of a bar magnet. There is, however, no desaturation, nor blinking, as have been described for a discrete parafoveal patch.

If a grey ring is overlaid on a field of red and green halves so that the vertical division between them bisects it, the Wertheimer effect is obtained: the ring either as a foveal or as a parafoveal stimulus shows no induced color (Figure 2, *E*). If such a ring, on a dark background, has a small portion isolated by opaque strips

(Figure 1, *G*), the ring fills itself out in very much the same way as the bar in Figure 1, *E*. As a late stage in the negative after-image there is an intermittent filling out of the whole figure into a solid dark disc on a brighter background. As before, the filling out of the ring, as a ring, is initial if the small isolated square is the foveal stimulus and late if it is the parafoveal stimulus. The solid-disc stage is earlier and more persistent as the width of the ring is increased at the expense of the inner opaque circle. If the diameter of the latter is as little as five millimeters, the foveal after-image of the opaque center does not appear. As a parafoveal stimulus it appears initially, and is later filled out, but may recur one or more times. If this small central disc is made translucent and of a contrasting color, instead of opaque, it fails to appear as a foveal after-image, but appears briefly, at first, as a parafoveal after-image. In the former case, there is a drift in the after-image that portrays the individual's "permanent personal tendency" without the component introduced by the Rexroad arrangement. In the Wertheimer ring, described above, the intact grey ring yields a whitish or yellowish negative after-image on appropriately (complementarily) colored fields. If the ring has been divided vertically into halves, however, and the halves staggered as in Figure 2, *E* or pulled apart so as to be surrounded, each by its colored field, a reddish color will be induced in the half in the green field, and a greenish color in the half in the red field. Moreover, in the negative after-image these induced colors are represented by complementary hues. The same effects can be obtained by laying an opaque strip along the division line over the whole figure. With the staggered halves the negative after-images of the halves show the peculiarity of appearing in alternation, one reaching its best definition as the other fades out, and vice versa. This effect appears to be independent of the point selected for fixation.

The filling-out process, in which a negative after-image overflows its bounds, pushes through the space between it and another after-image, and "captures" or pervades the latter, can be observed in several arrangements. There was not a single report of this process in the Rexroad arrangements and in the variations of right and left squares described above in *Size and Intensity,* nor in Figure 1, *C*. If, however, the small square in Figure 1, *F* is fixated, the initial negative after-image shows a solid sector filled in with the color

of the larger member, then the tip of the sector fades out, and later the negative after-image of the smaller member appears for a brief time. If a point in the larger member is fixated, the process is reversed: the filling-out occurs late instead of initially. In either case the last visible negative after-images are usually the filled-out single figure.

In Figure 2, *B* if any one of the small circles surrounding the large one is fixated, it will not produce a negative after-image, though the after-images of the other small circles appear definitely, simultaneously, and in alternation with that of the large circle. Usually this ring of small circles is the first organized after-image visible. In late, faint stages the small circles blend together into a fairly continuous ring. These effects appear even when the brilliance of the small circles is reduced far below that of the large one. During impression the large circle quickly loses in saturation and brilliance, and various ones of the small ones blink off and on in an irregular way. Initially in the period usually described as "blur" most subjects describe a crescent-shaped space filled in with a multiplication of small circles on the side of the large circle opposite the fixation-point. If the center of the large circle is fixated, there is in the first after-image stage a uniform multiplication of small circles, so that they appear to stand "three deep." Then follow geometrically correct reproductions of the stimulus pattern, with the large circle alternating with the ring of small ones in irregular successive arrangements. A striking late phase is reported by some observers in which the small circles tend to group in square clusters at four points suggesting the corners of a large square, so that the whole effect of circularity is lost.

If a circular space of about 50 mm. diameter is filled in with alternating opaque and translucent colored rings of three millimeters or less in width, and the central, smallest disc is fixated, there is in impression a shifting, dimming, and fusion of peripheral rings, sometimes with an apparent circular motion in one direction, clockwise or counterclockwise for a given observer. After impression there is a brief initial stage of rapid change of such complexity that most observers express surprise, pleasure, and interest. The order of appearance of after-images is from outside in. Circular forms grow out of lozenge-shaped matrices. Early forms have zigzag edges like circular saws. Those who noticed movement in impression reported movement in the opposite direction and at a faster rate in

the after-image. If an off-center fixation point is taken, the after-effects are at first eccentric, difficult to describe, but somewhat fan-like, like a checker-board in deep perspective, etc. Next comes a series of changes in which almost any combination of fusions of adjacent rings may be made out. Limiting cases are a correct reproduction of the stimulus and a single disc, but these are rare.

In Figure 2, *D*, if the central circle is fixated, figure-groups appear both in impression and in negative after-image. In impression there is the familiar flux of patterns observable on a more elaborate scale in McDougall's dot figure. One such pattern is the dot-*X*-dot; another, the twin-lozenge; another the dot-in-hexagon; another the two-three-two rows; another, one in which the circles become the background and the opaque separating material the figure: the "spider" figure, etc. These like most ambiguous figures can be voluntarily found or may emerge involuntarily, and no one of them can be indefinitely held. In after-image there is again a flux of patterns, with some differences. One, two, or more of the circles may disappear or be present in distinctly fainter intensity than the others; this is particularly true, and for greater durations, of the fixated circle. No pattern emerges in after-image that was not observed in impression—an observer does not usually perceive all possible patterns in the standard fixation time of 20 seconds unless instructed. The patterns take on an effect of objective reality; the circles in the hexagon-dot figure, for example, actually blend into a hexagon, which is later simplified into a circle, and the figure is finally a ring-around-a-dot; in the spider figure the outside boundaries of the outer circles become indistinguishable, etc.

In Figure 2, *C*, if the right-hand figure is fixated at *X*, one of the after-image stages is the complete rectangle filled out in homogeneous color. In the left-hand figure the large and small patches and intervening space are approximately equal to the corresponding ones in the right-hand figure, but do not together suggest any familiar geometrical form. After fixation at *X* in the left-hand figure, however, there is no after-image in which the two parts are blended into one homogeneous whole.

If the center of a square stimulus-patch is fixated for 20 seconds and the square is then rapidly turned through 45 degrees on the fixation-point as an axis, and the fixation maintained in the new arrangement for four or five seconds longer, there is at

first a negative after-image of the second stimulus, but this then becomes a circular disc with poorly defined edges, next a faint after-image of the second stimulus superimposed on a more distinct one of the first, and finally one of the first stimulus alone. This suggests a visual analogue of the primary law of Jost.

Late in the course of after-images from a stimulus-pattern containing two parafoveal stimuli there is a blurring or tracking between the two, more easily discerned if they are relatively large and close together. This occurs either across the fovea or not. Thus, two squares become a long rectangle, two circles a single figure with rounded ends, etc. Sometimes the track is seen as fainter than the figures which it joins. A late stage in the negative after-images from the large square in the standard Rexroad arrangement is a horizontal spreading with loss of definition of the right and left vertical edges and upper and lower edges kept distinct.

If two bars such as Figure 1, *E* are arranged at right angles to form a Greek cross, the after-image of the vertical member predominates over the horizontal. Although the complete figure is sometimes present and either the vertical or horizontal member may fade entirely out for a brief time, the vertical bar is present for a greater fraction of the time, and often persists complete when only the outer ends of the horizontal are present. If the figure is turned through 45 degrees so that neither crossbar is nearer vertical than the other, neither has the advantage, and competition between them yields alternations on equal terms. This difference in values between vertical and horizontal is seen in the *Phi movement* and in a number of illusions.

If a small square equal to the intersection of the bars is isolated with opaque strips, it behaves as a foveal or parafoveal stimulus in the way described above for one bar. In the Greek cross arrangement either the horizontal or vertical member may complete itself across the gap, or neither, or both. More frequently the vertical member is complete. If the figure is now turned through 45 degrees, a single one of the four arms may flow into and occupy the space corresponding to the small isolated square, leaving the other three short. Which of the four captures the small square in this way does not seem to be regularly determined.

In Figure 2, *A* the black bars are opaque strips inclosing luminous squares. During the fixation of one intersection of the bars

the "linoleum" illusion of small lighter squares at other intersections presented parafoveally appears. These illusory patches are intermediate in intensity between the bright squares and the dark bars, and are colored by stray light from the squares, especially parafoveally. Two unpredictable results from stimulation by this figure were noted. If, for example the bright squares are the Hering red No. 1, for a brief time initially the negative after-image of the whole rectangle appears as green, but with small red squares of size and separation appropriate for the intersections of the dark bars distributed over it. These are brighter than the green ground, and of brief duration. Later the negative after-images of the dark bars appear as lighter green lines, distinctly wavy rather than straight, and with different and constantly shifting intensity-values, so that they appear like vertical and horizontal threads located at different distances from the observer; in the end only a homogeneous green figure remains.

Time Relations. The after-image from a parafoveal stimulus, in comparison with that from an equivalent foveal stimulus, rises earlier, reaches its maximum of definition sooner, and fades more quickly, disregarding fluctuations. Its "life" is distinctly shorter. Under certain circumstances, as in Case 7 in *Size and Intensity* above, it may not appear with a positive after-image from a foveal stimulus, and then appear when the negative of the latter does. In such a case its duration is curtailed, apparently by the length of the positive stage of the foveal after-image. This brevity of life of the parafoveal image in general matches the more rapid desaturation and disappearance of a parafoveal patch when a steady fixation is maintained. So long as a "foveal stimulus" is a well-defined figure of homogeneous color, it follows this temporal course even if it greatly exceeds the area of the fovea and parts of it are at greater distances from the fixation-point than the parafoveal stimulus.

Color. All observers reported that immediately on the cessation of stimulation they saw, irrespective of the color of the stimulus, a pinkish or reddish diffuse luminous spot somewhat larger than the stimulus-figure. As this faded the after-image, positive or negative, rather gradually took form. Parafoveal stimuli at a distance of ten centimeters or more from the fixation-point produce immediately on the cessation of stimulation a brief flash of positive complementary after-image, extraordinarily rich and brilliant. This is

lost at once in the initial blur. Either a foveal or parafoveal negative after-image emerges as a dark, patterned film, floating on a paler ground called "haze," "fog," or "gas." With time it fades, going through a flight of colors that includes several other hues than the complement of the stimulus. Positive phases may alternate with negative. A stimulus of Hering red No. 1 gives a dark blue-green after-image in the first negative phase; this then becomes very dark blue or black, then violet, then rosy purple or carmine. Positive phases, if they appear at all after the first well-defined negative, are pale verdigris. The contrasting fog for negative phases is also of this color. Thus it is not possible to say that the negative after-image is consistently complementary, nor that the complementary image cannot be positive. Individual variations in reporting these changes are much reduced with practice.

When alternating red and green standard stimuli are presented at foveal and parafoveal positions, the rate of alternation necessary to produce an impression of yellow is markedly less for the parafoveal than for the foveal stimulus. Even when the rate of alternation is too slow for fusion in either case, the color of the negative after-image is appropriate to the yellow of mixture rather than to the last color presented when the stimulation is terminated, down to alternations as slow as one every two or three seconds.

Discussion

Dimmick (5) accounts for the uniform blue-gray-white of the after-images of parafoveal stimuli obtained under his conditions, described above, and for the absence of after-image from his central stimulus-disc and from a red parafoveal stimulus on the basis of the photochemical action of rhodopsin. The images are positive. "Under the prior dark adaptation this substance (the visual purple) has been generated in the extra-foveal region. The short, intense stimulation has produced a bleaching of the visual purple over the area stimulated, except where the stimulus is red or where it falls within the fovea. At the same time the effect on the more obtuse cones of the same area has been masked by the greater effect of the more sensitive visual purple." The positive after-images are assumed to be rod-effects. These statements carry the implication that rhodopsin is not generated in the fovea in dark-adaptation, therefore is not present to be bleached by the central light stimulus when it is pre-

sented. This explanation will not hold for the suppression of the negative after-image by parafoveal stimuli of certain sizes, as reported in our study, however, for these are presented in such intensities and for such a length of time as to make them fall within the conjectured province of the cones. This is further substantiated by the lively color-effects. More convincingly still, the standard Figure 1, *B* pattern may be set up as colored squares on a white background and fixated as usual for twenty seconds, but in ordinary diffuse daylight; if the after-images are "projected" on a plain white ground, the same effects will be obtained as described above.

It must be noted that under our conditions the negative after-image of as large a foveal stimulus as Dimmick's central disc (two centimeters in diameter) was not absent, and was absent by Dimmick's account only when the duration of impression was less than two seconds. These facts lead to the conjecture that the negative after-image may be an extra-foveal function, but may be produced by stimuli slightly less in extent than the fovea if protracted or intense, perhaps by stray light or irradiation. The negative after-image of a stimulus larger than the fovea in visual angle (thus constituted of both foveal and parafoveal parts) is filled out, but its delay in appearance may bear some relation to the advantage the non-foveal parts have, and the necessity for an equilibrating process to produce a homogeneous image. Further, the superior stability of the negative after-image may represent an experientially acquired visual conquest of adverse conditions in somewhat the same sense that the binocular parallax, which in its own right ought to blur and confuse bidimensional vision, has been met with particular complex readjustments of response to visual data which constitute the factor of tridimensionality in the seen world. At any rate it seems likely that impinging light patterns are retinally reconstellated in agreement with differing foveal and parafoveal characteristics as a part of the whole process of organization of figure and field, to which organization the experiencing individual simultaneously contributes and responds. Just as organization is an after-effect of stimulus-impact, so are after-images and memory-traces after-effects. It is possible that the visual process instead of maneuvering the physical parts of the eye into that arrangement which will furnish the best static photograph of the object to be seen at the instant of perception is engaging in a far more complicated and continually fluid search

for sequences of foveal and parafoveal relationships which are best for those temporally protracted appropriations to which we give the names retention and memory. It is undeniably a blessing that we are able to forget the vast majority of our experiences, but we need not think of the sense-organs as the agents of the forgetting.

We are unable to agree with Katz and Braddock that the negative after-image is formless. It is soft, thick, textureless, and hazy in contour, but it does appear in certain forms. These forms appear to be subject to certain regular principles of modification, unanticipated by an uninstructed observer, but uniformly reported. The modifying processes sometimes suggest the simplification of figures in accordance with the principle of Prägnanz, as in the seven-circle and rotated-square figures, sometimes the fluctuation of electrically polarized fields in the retina where attraction across a gap is followed by reversal of charge and retreat into the original position. An interesting theoretical question is raised as to what kind of process underlying the perception of negative after-image forms shall be subsumed. Two possibilities are available. Perhaps the more conventional, but less satisfactory, one is that after the cessation of stimulation local photochemical and electrical processes continue to form shifting patterns of excitation in the retinal photosensitive elements, which are carried as formed by afferent nerve processes "to consciousness." That is, consciousness watches the retinal display and responds to it in the way "it decides." Thus, in the seven-circle figure the emergence of pattern-groupings in impression would be assumed to depend on physical retinal attractions and repulsions, but limited in the amount of such groupings by the presence of the light stimulus-pattern. In the after-image this limitation is removed, and the flux of patterns takes on a quasi-objective reality. The contrary assumption presumes the possibility that the neural connections of the retina with the cortex may themselves carry surges of reversed polarity in either direction, and that both the retinal electrical fields and the ocular musculature are in the act of vision being organized not only by their own properties, but by those of a central or cortical organizer as well. This latter includes the effects of past learning. It seems inadvisable to assign a dominant rôle either to retinal or to cortical organizations, as they are in collaboration rather than in competition, and are perhaps functionally not two distinct processes.

This assumption is supported by the observation that no reports were given of a negative after-image pattern in the seven-circle figure which had not already been perceived as a pattern in impression. It also furnishes a putative basis for the mechanism responsible for the intimate relationship between after-image, eidetic image, and memory image. Considering the emphasis laid by the usual text on neural anatomy on the fact that the optic nerve is, strictly considered, a brain tract rather than a nerve, it does not appear to be physiologically untenable. The ease with which subjects were able by practice to reduce the amount of troublesome movement and to perceive definite pattern effects in the early stages of the after-image which the naïve subject tends to report as blurred and confusing defies explanation in purely retinal terms. Definite improvement was also shown in observing the late, faint stages of the negative after-image and in following the successive color-phases from start to finish. It is improbable that practice sensitizes the retina to negative after-images. Its influence must rather be exerted on the organizing and executant activities of the individual, and these we may suppose to be cortically controlled.

The saw-tooth effect in the negative after-image of concentric rings and the effect of bright dots in nodes derived from a thin bright line suggest the possibility that, when excitation over a limited area of the retina is at a certain low degree of intensity, the cortical summating and interpreting mechanism becomes inoperative, and that these effects represent a projection of the actual retinal-element mosaic, with the cells most affected by the residual excitation furnishing the bright spots and neighboring ones the surrounding lines. This might happen in a fashion similar to that in which corpuscles in the retinal capillaries are seen against the sky. The discrepancy between these effects and the preceding round rings and straight bars gives a clue to the character of the organizing work done by the cortex, and an idea of the difference there would be in the seen world if consciousness actually watched retinal displays as a passive spectator.

Every figure observed has yielded evidence that the negative after-image is an independent phenomenon rather than an obverse recovery phase from the positive. The latter is unvarying in form, except for a final shrinking and dimming. The goodness and duration of the positive predict in general little or nothing of the character of the

succeeding negative. There are often alternations of negative and positive which, on the fatigue-and-recovery assumption, would argue that there were phases of over-recovery with succeeding recovery from recovery, without additional stimulation. This is scarcely a convincing picture. Again, the positive after-image, supposed to result from a stimulation intense enough to persevere with a kind of momentum after the physical impact of light has ceased, is in some cases followed by no negative, as we have shown. McDougall (3) has shown that border-conditions can be the determining factor in setting up a positive or a negative after-image, irrespective of intensity. Jaensch (2) asserts that interest may also be, under certain conditions. Furthermore the fatigue-assumption is not substantiated by the hue of the negative after-image, presumptively complementary to that of the stimulus. The most definite and lasting negative from the Hering red No. 1 stimulus, for example, was a blue-black; a pale verdigris appeared now and then as positive. At the time of transition the positive appeared as a glossy film or spot over the surface of the negative; sometimes it disappeared by "winking" out, sometimes by diminishing gradually with the undiminished negative in view as if behind it. The like-colored small spots in the negative after-image of the linoleum figure (Figure 2, *A*) are puzzling. They may be regarded either as negative after-images of an induced green which was peculiarly exaggerated at the intersections of the opaque bars or as positive after-images of a red so unusually effective that it produced a positive after-image at the same time that the bright squares which were the source of all the available light-energy produced negative. The first explanation is counter to the experience of a distinct reddish glow cast over the opaque bars during impression, and leaves unexplained why there should not be red after-images of the bars between their intersections. The second is at first glimpse self-contradictory until the possibility is considered that field conditions as well as raw intensity may determine the appearance of a positive after-image; if this is true, they determine its appearance actually contrary to intensity in this case.

These phenomena, together with the observation that the life of the negative after-image is diminished proportionately as the life of a positive after-image from a given stimulus is increased by variation of other factors than intensity, hint that the negative after-image has its inception with the cessation of the stimulus, and is masked by the positive as long as the latter is present.

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QUELQUES CARACTÉRISTIQUES DES STIMULI PARAFOVÉALS ET LEURS IMAGES CONSÉCUTIVES

(Résumé)

Certains effets dans le comportement des images consécutives observées dans des conditions favorables ne suivent pas la route qu'on pourrait prédire *a priori* sur la supposition que l'image consécutive est une excitation rétinienne locale ou état qui persiste. On montre que les images consécutives qui résultent de la stimulation fovéale sont influencées dans leur intensité, leur couleur, et leur durée, et surtout dans la modification progressive de forme laquelle elles subissent, par la présence d'autres images consécutives qui résultent de stimuli parafovéals présentés simultanément. On montre que celles-là souffrent, comme résultat de la présence de celles-ci, une décroissance d'intensité et de durée laquelle dépasse la décroissance soufferte quand elles sont présentées seules. Il peut y avoir une oblitération complète de l'image consécutive négative du stimulus fovéal. L'aire du stimulus parafovéal se montre le facteur efficace dans la production de cet effet affaiblissant.

Quelques-uns des effets observés semblent être déterminés par les activités perceptives de l'observateur. Les simplifications de la forme et le remplissage de formes entières de parties discrètes ont lieu selon le principe de précision. Dans quelques cas on peut observer des effets qui suggèrent l'absence momentanée de l'organisation corticale dans l'expérience visuelle. On suggère que l'image consécutive négative est indépendante de la positive à l'égard de l'état.

KNIGHT

EINIGE EIGENSCHAFTEN DER PARAFOVEAREIZE UND IHRE NACHBILDER

(Referat)

Gewisse Wirkungen im Verhalten der Nachbilder, die unter günstigen Umständen beobachtet wurden, folgen dem Gang nicht, der *a priori* auf dem Grund vorausgesagt werden möchte, dass das Nachbild eine dauerende, örtliche Netzhautreizung oder ein Zustand sei. Nachbilder, die durch Foveareizung erzeugt werden, können durch ihre Intensität, Farbe, und Dauer und besonders durch die fortschreitende Veränderung der Form, die sie durchmachen, durch die Anwesenheit anderer Nachbilder, die aus Parafoveareizen gleichzeitig auftreten, beeinflusst werden. Als Folge der Anwesenheit der letzteren zeigen sich die ersteren eine Abnahme der Intensität und Dauer, welche im Überschuss der erlittenen Abnahme ist, wenn sie allein dargeboten werden. Es kann eine vollkommene Verwischung des negativen Nachbildes durch den Foveareiz stattfinden. Die Fläche des Parafoveareizes zeigt sich als ein wirksamer Faktor bei der Erzeugung dieser abschwächenden Wirkung.

Einige der beobachteten Wirkungen scheinen durch die Wahrnehmungstätigkeiten des Beobachters bestimmt zu sein. Vereinfachungen des Musters und die Ausfüllung der Ganzformen aus getrennten Teilen kommen in Übereinstimmung mit dem Grundsatz der Genauigkeit vor. In einigen Fällen können Wirkungen beobachtet werden, die auf eine augenblickliche Abwesenheit der Rindenorganisation in der Gesichtserfahrung andeutet. Es wird vorgeschlagen, dass das negative Nachbild unabhängig an Stand vom positiven sei.

KNIGHT

AN INTERPRETATION OF THE RELATION BETWEEN VISUAL ACUITY AND LIGHT INTENSITY*

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INTRODUCTION

The resolving power or acuity of the eye depends in a very large measure on light intensity. This is a fact of much theoretical as well as practical interest, and a variety of theories have been proposed to account for this dependence (13). In the present paper we shall present, first, a new experimental study of the acuity-intensity relation in a number of its different aspects, and, secondly, a hypothesis which furnishes a quantitative basis for the understanding of this relation.

In the measurement of visual acuity, one determines the smallest visual angle at which two neighboring contours may be perceived as discrete. By definition, the acuity is taken to be inversely proportional to this minimal angle or resolution threshold. Now, as we have pointed out (13), the resolution threshold is influenced by a complex variety of factors. It depends on the intensity of the test object and the intensity of the background on which the test object appears. Among other things the threshold also depends on the size and form of the test object.

If one is interested in the effect of intensity upon acuity, one must bear in mind that intensity cannot be treated as a unitary variable for the purpose of this problem. There are two significant intensity variables involved, namely, the intensity of stimulation offered respectively by the test object and that of its background. The most satisfactory procedure would then involve independent variation of these two factors. Moreover, it would seem highly desirable to eliminate, as far as possible, variation in any of the other factors which affect acuity so that the specific influence of intensity may be thoroughly isolated.

In researches in this field it has been customary to follow a pro-

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cedure which is typified by the well-known experiment of König (10). König employed a black test object (Snellen "hook") on a white background, a single source of light illuminating the whole. At a given level of illumination, the acuity was measured by diminishing the visual angle subtended by the entire test pattern until its details could no longer be distinguished. At the least, three variables, intensity of stimulus, intensity of background, and size of stimulus, were simultaneously altered. It is evident that the conditions were too complicated for the results to be clearly interpreted.

Perhaps the most serious objection to König's data is this: That the resolution thresholds were determined by diminishing the size of the retinal image of the entire acuity pattern and not merely the "gap" to be resolved. It has been shown very clearly by a number of observers that a change in size of the test object, other things being equal, affects the acuity in a very decided manner. It must be concluded, therefore, that the data of König do not furnish an unequivocal description of the manner in which visual acuity changes when illumination alone is changed. In order to obtain such a description, one would have to maintain a constant size of the test object, changing only the space which is to be resolved.

Apparatus and Method

In the present experiments, an effort was made to simplify the situation to the greatest possible degree. The test object employed consisted of two narrow, parallel, vertical bars of constant size but variable separation. The acuity was tested by determining the least interval between the bars which would yield a perception of doubleness. The bars and their background were illuminated by separate light sources.

The behavior of the resolution threshold was studied under a variety of conditions. In the first series of experiments (*Series A*), the background on which the bars appeared was completely dark; the intensity of the bars was varied through a considerable range. In the second series (*Series B*), the bars themselves were dark while the intensity in the background was varied. Still other series (*Series C-F*) of experiments were made in which both test object and background were illuminated.

It will be clear from the results that it is impossible to think of any constant, unique relation between visual acuity and illumina-

tion. The relation which one empirically discovers is, in very large measure, a function of the concrete experimental situation involved. The advantage which we find in the method employed here is that it permits a clearer analysis of the problem and a much better insight into the complicated relationship between intensity and acuity than do the methods hitherto used.

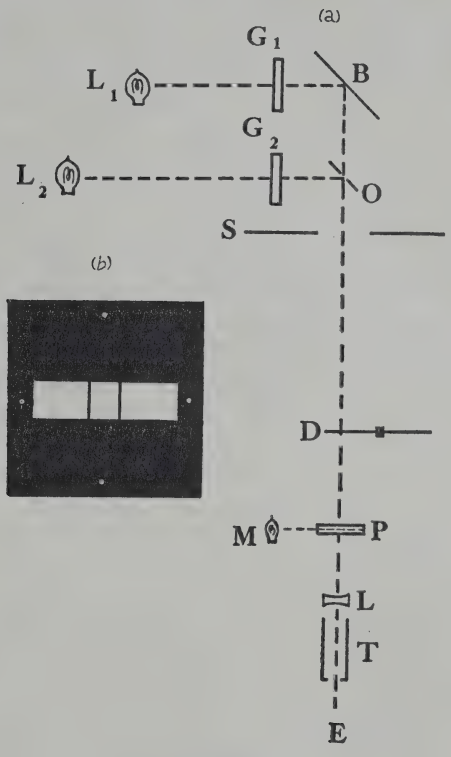

FIGURE 1
DIAGRAM OF APPARATUS USED FOR THRESHOLD AND IRRADIATION MEASUREMENTS

The apparatus is pictured in Figure 1 (*a*). Test object (*O*) is furnished by two narrow strips of polished metal (stainless steel) placed at 45° to the line of sight and illuminated from the side, thus functioning as mirrors. Behind these strips is a large metal mirror (*B*), also placed at 45° and illuminated from the side, but

by an independent source; this mirror serves to furnish background illumination. A concave lens (L) is placed in front of the eye so that the pattern actually seen by the observer is a much reduced virtual image of the two bars, 33 cm. from the eye. This image is then the effective test object. The image of each bar always subtends the same angular dimensions at the eye (width, 2′ 22″; height, 20′). A rectangular aperture in the screen (S) limits the size of the background field to 70′ width and 20′ height. Thus, provided background illumination is supplied, the observer sees a small rectangular field in the center of which are two vertical stripes. The experimenter, by manipulating a fine screw, can alter the distance between the metal bars (O) and thus test the observer's acuity.

The intensity reflected by these bars can be controlled by varying the distance of the lamp (L_2) illuminating them, and also by inserting one or more ground glasses (G_2) in front of the lamp. The background intensity is independently controlled by a similar method. The two lamps (L_1 and L_2) are Mazda show case lamps rated at 110 volts and 25 watts. The voltage across each lamp was maintained at constant level of 100 volts throughout all the work, being carefully checked by a voltmeter. The physical quality of the light was thus constant. There was no sensible color difference between test object and background illumination.

The sector disc (D) is used as a supplementary means of obtaining intensity variations.

Each of the lamps is movable along a track bearing a scale upon which can be read the distance of each of the lamps from its corresponding mirror. The background illuminating system was calibrated by the following method: a Macbeth illuminometer was pointed at the background with two or more ground glasses (G_1) placed in front of the lamp (L_1) to provide diffuse illumination. Illuminometer readings in apparent foot-candles were then taken for a number of representative positions of the lamp and a curve plotted from the results. It was then a simple matter to obtain any intensity desired.

Calibrations having once been obtained for the background system, the test object illuminating system could then be calibrated by obtaining equality-of-brightness settings with the background illuminating system. Such settings were very easily and accurately made

since they merely involved the adjustment of the illumination so that the whole test pattern appeared as a homogeneous rectangle.

The observer viewed the test object monocularly through the tube (T). At the eye-end of this tube there is a square aperture 2 mm. on a side which serves as an artificial pupil, thus eliminating the effect of variations in the size of the natural pupil both upon the retinal illumination and upon the definition of the retinal image. The effective pupillary area thus being constant and of known value, the retinal illumination can be calculated for any given brightness of the reflecting surface.

Foveal fixation is secured by centering the image of the test object between four red control points made by drilling four indentations in the plate glass (P) and illuminating the whole from the edge with red light from the ruby lamp (M). The control points are so placed in respect to a circular opening in the lens end of the tube that a movement of the eye in any direction would cause one or more of the points to be obscured by the edge of the aperture. The red color was intended to give the control points attention value even though their intensity was barely over the threshold. By the use of this method the observer can easily, with practice, maintain accurate foveal fixation of the test object. The appearance of the test object with the control points in position is given in Figure 1 (b).

In the tests of acuity, the following procedure was adopted: the observer was dark-adapted for five minutes, experimentation having shown that the threshold remained practically constant after this period. The instructions to the observer were: "Center the image of the test object between the four red control points. You are to report by striking the sounder when you first see two bars instead of one. Try to keep as constant a criterion of resolution as possible." Particular care was taken before each observation to be sure that the eye was properly aligned with the artificial pupil, the control points, and the test object. Starting with the bars very close together so that they appeared as a single stripe, the experimenter gradually increased their separation until a gap was first perceived. Only the ascending threshold was used because of its great reliability and consistency.

Although the test object employed has the advantage of great simplicity from the viewpoint of theory, the threshold determina-

tions are very difficult for an untrained observer. While, in the case of such objects as Snellen hooks, Landholt rings, etc., the details are resolved rather suddenly; in this case the resolution is a gradual process and the observer at first has difficulty in identifying the exact moment at which he first sees two bars instead of one. Thus, the conditions are analogous to those in experiments on the two-point threshold in tactual perception where much depends on the criterion of duality. Like the touch threshold, the resolution threshold is markedly affected by practice. For that reason, thoroughly trained observers are necessary before consistent results are obtained. However, we preferred the two-bar test object, despite the practical difficulty involved in its use, because it permits the size of the test object to be held constant.

Experience shows the threshold to be variable at first, but, with practice, it is much less variable as well as lower. In order to illustrate the effect of practice a systematic study was made with one observer. Three series of ten observations each were made on each of ten successive days. The drop in the threshold was made from 105″ to 23″ or to about one-fourth of its original value; the threshold then remained constant at this lower level.[1] It is likely that the improvement in the threshold has the same basis as in the corresponding tactual two-point threshold, namely, the adoption of a finer criterion of doubleness.

Two thoroughly trained observers were used in the experiments proper, Mr. C. W. Clendenon (*C*), a graduate student at the University of Kansas, and the writer (*W*).

Experimental Results

Series A (Test Object Illuminated, Ground Dark). In the first series of experiments, the test object alone was illuminated and appeared as two bright vertical bars on a dark ground. The test object intensity was successively given a number of different values. The threshold was then measured at each intensity. The mean threshold values, the mean variation, and the number of observations are given in Table 1 for each observer under the "Empirical

[1]Volkmann (12, p. 88) also found a very large increase in visual acuity with practice.

TABLE 1

SERIES *A* (TEST OBJECT ILLUMINATED, GROUND DARK)

Test object intensity		Empirical threshold			Irradiation (2*d*)			Theoretical threshold
Photons	Log photons	No. obsns.	Mean	M.V.	No. obsns.	Mean	M.V.	
			Observer *C*					
0.38	—0.42	200	135″	7″	100	63″	6″	130″
0.60	—0.22	120	94	3	100	25	4	92
1.00	0.00	170	77	2	100	12	7	79
1.90	0.28	90	67	2	100	0	5	67
3.40	0.53	100	57	5	100	8	3	75
6.20	0.79	100	61	2	100	6	4	73
21.6	1.33	100	79	2	100	21	4	88
33.0	1.52	100	106	5	100	31	5	98
49.0	1.69	100	114	7	100	46	5	113
			$S_0 = 67''$					
			Observer *W*					
1.12	0.05	100	79″	9″	100	28″	10″	86″
2.25	0.35	75	64	6	100	4	4	62
3.40	0.53	100	61	4	100	— 1	5	57
6.20	0.79	100	60	4	100	— 3	5	55
9.00	0.95	100	58	7	100	0	4	58
12.6	1.13	100	61	2	100	7	5	65
17.6	1.25	100	62	5	100	10	4	68
21.6	1.33	100	67	4	100	23	2	81
33.0	1.52	100	67	4	100	29	5	87
48.0	1.68	100	88	2	100	33	4	91
52.0	1.72	100	100	3	100	58	3	116
			$S_0 = 58''$					

Threshold."[2] Figure 2 represents the threshold variation as a function of the logarithm of the test object intensity in photons for one observer (*C*). The mean threshold values are represented by white circles;[3] each circle represents approximately 100 observations.

The results indicate that a rather peculiar relationship holds between the threshold and intensity. At low intensities the threshold is very high. As the intensity is increased, the threshold decreases but only to a certain minimum after which it *rises*. Thus the prevailing assumption that increased intensity is always favorable to acuity is incorrect; under certain conditions the acuity actually decreases with high intensities.

[2]The last four columns in the tables refer to measurements described in the next section.

[3]The black circles and heavy curved line in the figure refer to measurements described in the next section.

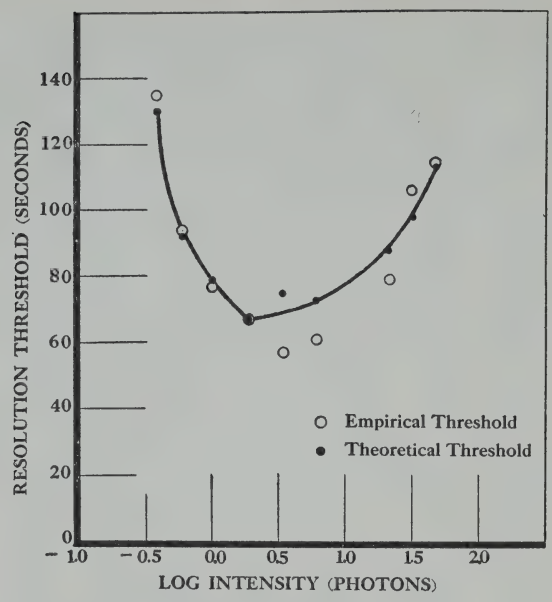

FIGURE 2

SERIES *A* SHOWING THE VARIATION IN THE RESOLUTION THRESHOLD WITH INTENSITY OF THE TEST OBJECT, IN THE CASE OF BRIGHT BARS ON A DARK GROUND
(Observer *C*)

Series B (Dark Test Object on Illuminated Ground). In *Series B* the test object is light free and the light in its background is varied. The results are given in Table 2 and shown graphically in Figure 3 in which the resolution threshold is plotted as a function of the logarithm of the ground intensity.

In the present case, the threshold decreases continually with intensity, the rate of decrease becoming progressively less. This is the relation usually assumed to hold between the threshold and intensity; the higher the intensity, the lower the threshold. This rule was found to have only limited significance, however, in view of the results of *Series A* in which the threshold was actually found to increase with a high intensity.

It follows from these results that it is impossible to state a single type of law relating the threshold and intensity; the threshold de-

TABLE 2
SERIES *B* (DARK TEST OBJECT ON ILLUMINATED GROUND)

Ground intensity		Empirical threshold			Irradiation ($2d$)			Theoretical threshold
Photons	Log photons	No. obsns.	Mean	M.V.	No. obsns.	Mean	M.V.	
			Observer *C*					
0.26	—0.57	50	97″	11″	30	75″	1″	116″
0.38	—0.42	130	68	5	50	33	5	74
0.52	—0.28	100	56	4	40	14	6	58
0.68	—0.17	100	47	5	60	9	10	50
1.90	0.28	100	41	3	30	0	5	41
2.73	0.44	100	35	4	30	— 6	4	35
4.80	0.68	100	32	4	30	— 9	2	32
9.00	0.95	150	32	5	20	—12	2	29
13.6	1.13	100	29	5				
49.0	1.69	50	31	5	40	—16	1	25
66.5	1.82	100	32	1				
			$S_0 = 41''$					

pends not only on intensity but also on its *particular distribution* between test object and ground.

Ferree and Rand (3) have found significant differences between the resolution threshold for light objects on a dark ground and dark objects on a light ground. They attribute this difference, at least in part, to irradiation, a concept developed in the present paper to explain the variation in the resolution threshold with light intensity.

The results of *Series A* and *B,* as well as the interpretation of the variation in the resolution threshold in terms of irradiation, are described in a preliminary account of the present paper (14). Kravkov (11) conducted an experiment to test the results reported in the preliminary paper. He found the rise of the threshold in the case of illuminated bars on a dark ground but his attempt to test the irradiation hypothesis is rendered ineffective because he used much larger areas in the measurement of irradiation than in the experiments on the resolution threshold.

Hecht (5) has objected that the results found in *Series A,* as described in the preliminary account of the present paper, are not a true measure of visual acuity. His objection is that the bars are very narrow and bright, whereas, in the usual tests of acuity, a large portion of the retina is illuminated. As evidence for this view he states that when the intensity conditions are interchanged as in *Series B,* the usual type of relationship is found between the resolution

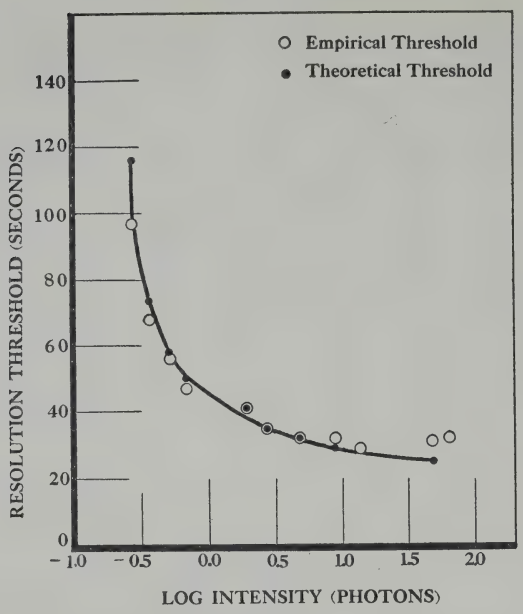

FIGURE 3

SERIES *B* SHOWING THE VARIATION IN THE RESOLUTION THRESHOLD WITH INTENSITY OF THE GROUND, IN THE CASE OF DARK BARS ON A BRIGHT GROUND

(Observer *C*)

threshold and intensity. He concludes that some phenomenon connected with glare accounts for the rise of the threshold in *Series A*.

Hecht's objection would be pertinent were it true that *Series A* represents an isolated case in which the resolution threshold increases with intensity. However, in other experiments (*Series C-F*), the same sized background is illuminated as in *Series B*, thus meeting Hecht's criterion for an adequate test object. In certain cases, the threshold of resolution rises with the difference in intensity; in others the threshold decreases with the difference in intensity, depending entirely on the particular distribution of intensity between test object and ground. On the basis of our results we are forced to conclude that glare is hardly an adequate explanation of the results found in *Series A*, since other series of experiments in which glare can play no part show results which are contrary to the idea that acuity always increases with intensity.

The Irradiation Hypothesis

What is the cause of the variation of acuity with intensity? During the course of the above experiments an incidental observation was made which suggested a possible answer to this question. It was noticed that the two bars of the test object, although physically constant in width, appeared to undergo pronounced broadening and narrowing effects as the intensity was altered. It is of course well known that a small bright object appears subjectively enlarged; this is the phenomenon commonly described as "irradiation." It seemed likely that the rise of the threshold at high intensities in *Series A* (test object illuminated, ground dark) was due to this factor. It was particularly interesting to note, however, that an apparent broadening of the bars occurred at *low* as well as at *high* intensities. Thus if one began with weakly illuminated bars, and gradually increased the intensity, the bars appeared very wide at first, then narrowed to a certain minimum only to broaden again as the highest intensities were reached.

These observations suggested a working hypothesis, namely: *variations in apparent width are the immediate cause of the changes of visual acuity with intensity.* With an apparent increase in width, the interspace appears narrower, and hence a greater physical separation is necessary for discrete perception. If the bars appear to diminish in width, the interspace appears wider, and a correspondingly smaller physical separation is required for discrete perception. In other words, the immediate effect of a subjective enlargement of the bars is to raise the threshold for resolution; a subjective narrowing of the bars lowers the threshold for resolution.

Figure 4 illustrates this hypothesis. In Figure 4 (*a*) the two rectangles drawn in solid lines represent the appearance which the bars would present in the absence of irradiation. The dotted contours in Figure 4 (*b*) represent the apparent enlargement through the influence of irradiation. It is assumed that if irradiation were absent the bars would have to be separated by a certain distance S_0 before the gap between them would be perceived; this is the "ideal" threshold. If, however, the bars, originally placed at a distance S_0, are increased in apparent width by irradiation, the gap between them will become imperceptible. If d is the apparent displacement of each contour, then the distance between the two physical stimuli will

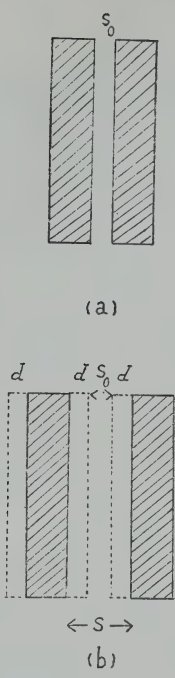

FIGURE 4

ILLUSTRATING THE EFFECT OF IRRADIATION ON THE RESOLUTION THRESHOLD

(Not drawn to scale)

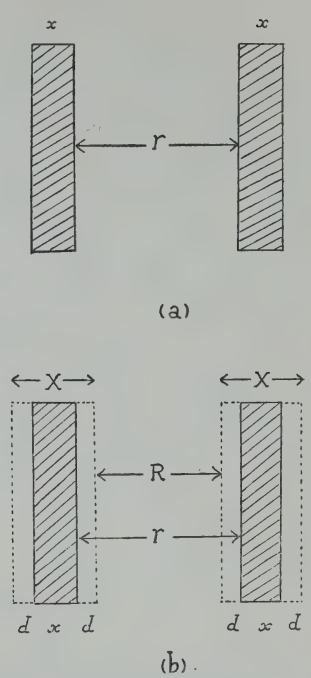

FIGURE 5

ILLUSTRATING THE THEORY OF THE IRRADIATION MEASUREMENTS

(Not drawn to scale)

have to be increased by an amount $2d$ before the apparent separation of the two contours will be restored to the value S_0 and the gap once more perceived. Under these conditions, the threshold of resolution will be

$$S = S_0 + 2d$$

Let us assume for the moment that the ideal value, S_0, of the threshold is known, i.e., the value which would be obtained if irradiation did not exist, so that the apparent sizes of objects would be proportional to their physical sizes, according to the laws of geometrical optics. It is then obvious that if we have means of measuring $2d$, the relative shift of the adjoining contours under any given intensity conditions, we can predict the value of the threshold

under these same conditions. We can thus subject our hypothesis to a strict quantitative test.

A measurement of irradiation was obtained by a modification of the method devised by Volkmann (12, p. 3). Employing the same apparatus used for the threshold determinations, and adjusting the illumination to the desired value, the two vertical bars were gradually separated by the experimenter until the observer judged that the width of the interspace appeared equal to the *sum* of the *widths* of the *two* bars. The discrepancy between the physical and subjective widths then furnished a measure of the apparent displacement of the contours. Figure 5 shows the principle of this method. In Figure 5 (*a*) the two physical stimuli and their physical separation r are represented, x being the physical width of each bar. Figure 5 (*b*) shows the appearance which the stimulus pattern presents to the observer. The bars are subjectively widened on each side by an amount d so that their apparent width

$$X = x + 2d$$

If R is the *apparent* separation of the inner contours, then according to the condition imposed

$$R = 2X$$

Furthermore,

$$R = r - 2d$$

Combining these three relationships, we find

$$2d = \frac{r}{3} - \frac{2x}{3}$$

Hence, if, after the observer has made the prescribed judgment, we measure the distance between the inner edges of the two physical stimuli, divide by 3, and subtract $2/3$ of the physical width of each stimulus bar, we have a numerical measure of the relative displacement of the two perceptual contours.

In this manner, one can make measurements of irradiation under the same conditions used in the acuity measurements. A comparison of these two independent sets of observations then reveals how close a correlation exists between acuity and irradiation. The results of such irradiation measurements are to be found in Tables 1 and 2; for each particular intensity, the computed value of $2d$ and the mean variation of this quantity are given.

In the case of *bright bars on a completely dark ground* (*Series A*),

d is large and positive at low intensities[4] as we have already noted. As the intensity is raised, d decreases to zero, and then increases once more.

In the case of *dark bars on a bright ground* (*Series B*), we note the striking fact that d is *positive* at the lower intensities of the background, in other words the dark bars are subjectively *widened*. This is the phenomenon described by Volkmann (12, p. 22 ff.) as "negative irradiation." This apparent enlargement of the bars was found to persist throughout a fairly wide range of intensities, although decreasing continually. Eventually an intensity was reached at which d became zero. Finally, at still higher intensities the bars decreased in apparent width, i.e., d became negative.

Returning to our expression

$$S = S_0 + 2d$$

we note that, even though d can now be numerically specified under any intensity conditions, we need to know the value of S_0 before we can test the quantitative validity of the hypothesis proposed.

It will be recalled that S_0 is the resolution threshold corresponding to a condition in which there is no apparent expansion or contraction of the bars, i.e., the quantity d is zero. The experimental data, which we have already described, yield a means of determining the value of S_0. For example, in the case of dark bars on a bright ground, we saw that d was positive at low intensities and negative at high intensities, passing through the value zero at some particular intermediate value (I_0) of the background intensity. If, returning to the data on visual acuity, we determine the value of the threshold for the resolution of the same dark bars at this same intensity (I_0) of the background, we obtain the numerical magnitude of S_0.

In every series of experiments it was found that d assumed the value zero at some point in the intensity scale. This made it possible to obtain the value of S_0 and then to calculate *theoretical values* for the thresholds on the basis of our equation. One could then determine how well the proposed hypothesis was confirmed by the threshold data obtained empirically.

These calculated thresholds are given in the last column of each

[4]The fact that small light objects on a dark ground irradiate outward at low as well as very high intensities appears in some data given by Volkmann (12, p. 24 ff.).

table, and can be compared with the empirical thresholds given in the same table. In the figures representing the data, the calculated thresholds are indicated by black circles, and a smooth curve is drawn through these calculated values. In the same figures, the empirical thresholds are represented by white circles. In view of the difficulty involved in the measurements of both the resolution threshold and apparent size, the agreement between theoretical and experimental values is surprisingly good. The theory finds confirmation not only in the results of *Series A* and *B*, but also in *Series C-F* in which both test object and ground were illuminated.

Experimental Results (*continued*)

Series C (Ground Intensity Constant at 3.4 Photons; Test Object Intensity Varied). In the remaining series of experiments, both test object and ground were illuminated. The purpose was twofold: (*a*) to test the irradiation hypothesis under a number of different experimental conditions and (*b*) to demonstrate, if possible, a number of distinct types of relationships between the resolution threshold and the degree of difference in intensity. The test object intensity was made successively greater than the constant ground intensity of 3.4 photons, the degree of intensity difference increasing with the test object intensity. The empirical and theoretical resolution thresholds were determined as before. The data appear in Table 3 and are represented graphically in Figure 6. The empiri-

TABLE 3

Series *C* (Ground Intensity Constant at 3.4 Photons; Test Object Intensity Varied)

Test object intensity		Empirical threshold			Irradiation (2*d*)			Theoretical threshold
Photons	Log photons	No. obsns.	Mean	M.V.	No. obsns.	Mean	M.V.	
			Observer *W*					
5.3	0.72	50	112″	4″	50	71″	5″	109″
6.4	0.81	50	58	5	50	14	5	52
7.3	0.86	50	52	3	50	5	4	43
8.8	0.94	50	37	2	50	1	5	39
10.8	1.03	50	35	4	50	— 2	6	36
13.6	1.13	50	38	2	50	0	6	38
16.8	1.23	50	40	2	50	8	1	46
21.6	1.33	50	49	3	50	12	5	50
49.0	1.69	50	66	4	50	22	3	60
			$S_0 = 38''$					

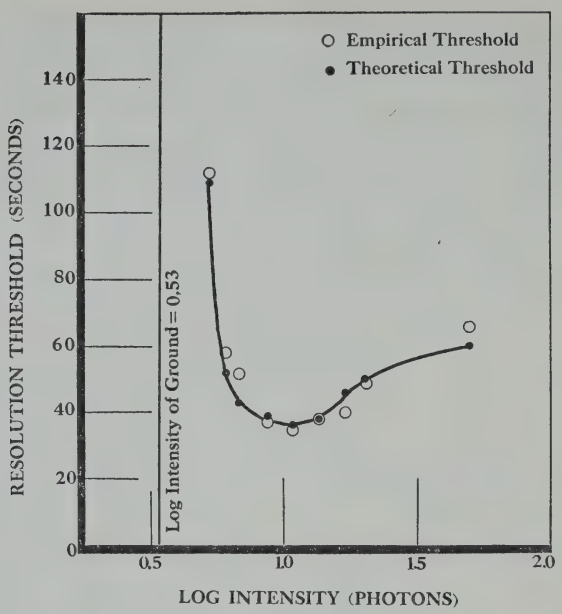

FIGURE 6

SERIES *C* SHOWING THE VARIATION IN THE RESOLUTION THRESHOLD WITH THE GROUND INTENSITY CONSTANT AT 3.4 PHOTONS, TEST OBJECT INTENSITY VARIED

(Observer *W*)

cal and theoretical resolution thresholds are plotted as a function of the logarithm of the intensities appearing in the test object; the logarithm of the constant ground intensity is indicated by the vertical black line at the left of the figure.

As the intensity difference increases, the empirical resolution threshold decreases to a minimum, the approximate value of which persists for a considerable range of intensity difference. With an extreme difference the threshold increases to almost twice its minimum value.

We now consider the irradiation measurements. At the lowest degree of intensity difference, *d* was large and positive. This apparent enlargement of the bars decreased with the increasing difference in intensity to a small or negative value which persists

for a considerable range and then gradually increases once more.

Returning to our expression

$$S = S_0 + 2d$$

we note that, even though d can now be numerically specified under any given intensity conditions, we need to know the value of S_0 before we can test the quantitative validity of our hypothesis. The irradiation measurements in *Series C* yield a means of determining the value of S_0. For example, in the irradiation measurements described above, d assumed a zero value at some intermediate intensity difference (I_0). If, returning to the data on the empirical resolution threshold measurements, we determine the resolution threshold at I_0, we obtain the numerical value of S_0. S_0 in this case was found to have a value of 38″. Substituting the values for S_0 and $2d$ in the equation given above, we find that the theoretical resolution threshold thus obtained first decreases to a minimum and then increases as the intensity difference increases. The minimum values persist for a considerable range of intensity difference. It will be seen that the agreement between empirical and theoretical resolution thresholds is good.

Series D (Test Object Intensity Constant at 3.4 Photons; Ground Intensity Varied). Test object and ground intensities are the converse of those used in *Series C,* thus allowing a comparison to be made of the effect of interchanging intensities. The results bear out the advantage of this method. The ground intensity was made successively greater than the constant test object intensity of 3.4 photons, the degree of intensity difference increasing with the ground intensity.

TABLE 4

SERIES *D* (TEST OBJECT INTENSITY CONSTANT AT 3.4 PHOTONS; GROUND INTENSITY VARIED)

Ground intensity		Empirical threshold			Irradiation ($2d$)			Theoretical threshold
Photons	Log photons	No. obsns.	Mean	M.V.	No. obsns.	Mean	M.V.	
			Observer *W*					
4.8	0.68	50	131″	1″	50	74″	6″	138″
6.2	0.79	50	92	10	50	16	1	80
8.6	0.93	50	64	2	50	0	4	64
13.6	1.13	50	55	3	50	— 5	1	59
21.6	1.33	50	46	1	50	—18	5	46
49.0	1.69	50	50	1	50	—28	1	36
			$S_0 = 64''$					

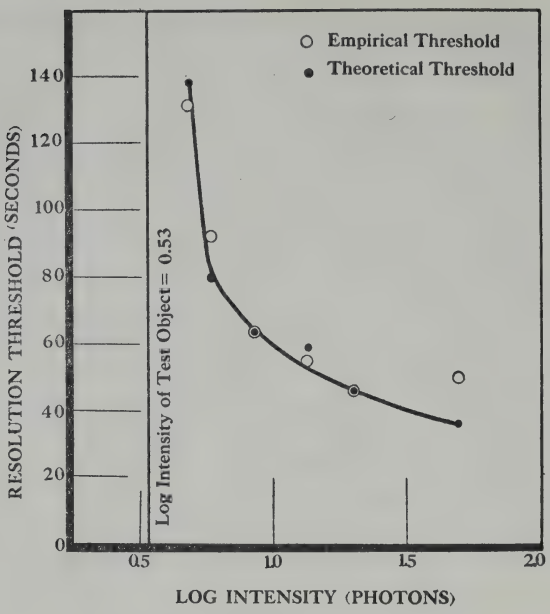

FIGURE 7

SERIES *D* SHOWING THE VARIATION IN THE RESOLUTION THRESHOLD WITH THE TEST OBJECT INTENSITY CONSTANT AT 3.4 PHOTONS, GROUND INTENSITY VARIED

(Observer *W*)

The data are given in Table 4 and graphically represented in Figure 7. The threshold is plotted as a function of the logarithm of the intensity in the ground; the vertical black line at the left of the figure gives the position of the logarithm of the constant test object intensity.

The empirical resolution threshold falls continually with the increase in the difference in intensity, the rate of decrease becoming progressively less and producing the relation usually said to hold between the threshold and intensity. Measurement shows d to be large and positive with a small difference in intensity; d then decreases in value, passing through a zero value and becomes increasingly negative. S_0 was found to have a value of 64″. The theoretical resolution threshold, calculated from the d measurements, decreases steadily with the increase in the intensity difference, the rate

of decrease becoming progressively less. Empirical and theoretical threshold values show a very satisfactory agreement.

Series E (Ground Intensity Constant at 49.0 Photons; Test Object Intensity Varied). The ground intensity was given a value of 49.0 photons and the intensity of the test object was increased in successive steps. The intensity difference, in this case, *increases* as the test object intensity *decreases.*

The data appear in Table 5 and are graphically represented in Figure 8 for observer *C*. The empirical and theoretical resolution

TABLE 5

SERIES *E* (GROUND INTENSITY CONSTANT AT 49.0 PHOTONS; TEST OBJECT INTENSITY VARIED)

Test object intensity		Empirical threshold			Irradiation ($2d$)			Theoretical threshold
Photons	Log photons	No. obsns.	Mean	M.V.	No. obsns.	Mean	M.V.	
			Observer *C*					
3.4	0.53	100	34″	6	100	— 4″	5	39″
6.4	0.81	100	43	5	100	0	5	43
8.8	0.94	100	50	5	100	10	4	53
13.7	1.14	100	58	4	100	22	10	65
22.5	1.35	100	81	9	100	30	18	73
44.6	1.65	100	151	2	100	99	5	142
			$S_0 = 43''$					
			Observer *W*					
4.4	0.64	50	37″	4″	50	—15″	4″	37″
6.4	0.81	50	37	5	50	—15	5	37
9.3	0.97	50	41	4	50	— 6	5	46
10.8	1.03	50	52	3	50	0	5	52
13.7	1.14	50	60	2	50	12	4	64
22.5	1.35	50	90	4	50	22	3	74
44.6	1.65	50	128	6	50	68	2	120
			$S_0 = 52''$					

threshold values are plotted as a function of the variable test object intensity. The position of the logarithm of the constant ground intensity is indicated by the vertical black line at the right of the figure. The empirical resolution threshold is high with a small intensity difference, then decreases steadily with the increase in the difference in intensity.

The irradiation measurements show that d was, at first, large and positive; d then decreases in value, passing through a zero value, and becomes increasingly negative. In this case, S_0 was found to have a

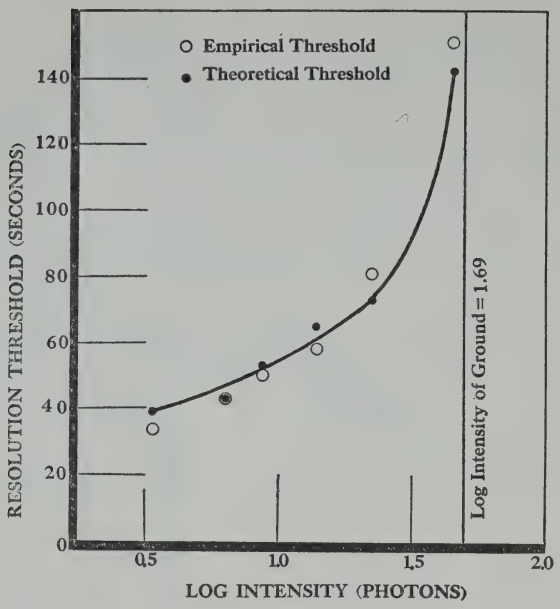

FIGURE 8

SERIES *E* SHOWING THE VARIATION IN THE RESOLUTION THRESHOLD WITH THE GROUND INTENSITY CONSTANT AT 49.0 PHOTONS, TEST OBJECT INTENSITY VARIED

(Observer *C*)

value of 43″. The theoretical resolution threshold calculated from the *d* measurements varies in a parallel fashion with the empirical resolution threshold.

Series F (Test Object Intensity Constant at 49.0 Photons; Ground Intensity Varied). The test object intensity was given a value of 49.0 photons and the intensity of the ground was increased in successive steps. The intensity difference, in this case, increases as the ground intensity decreases.

The data are given in Table 6 and graphically represented in Figure 9 for observer *C*. The empirical and theoretical resolution threshold values are plotted as a function of the variable ground intensity. The position of the constant test object intensity is indicated at the right of the figure by the vertical black line.

The empirical resolution threshold is high with a small difference

TABLE 6

SERIES *F* (TEST OBJECT INTENSITY CONSTANT AT 49.0 PHOTONS; GROUND INTENSITY VARIED)

Ground intensity		Empirical threshold			Irradiation ($2d$)			Theoretical threshold
Photons	Log photons	No. obsns.	Mean	M.V.	No. obsns.	Mean	M.V.	
			Observer *C*					
3.4	0.53	50	85″	5″	50	52″	4″	110″
4.4	0.64	50	82	4	50	28	2	86
9.8	0.99	50	58	2	50	0	5	58
13.7	1.14	50	55	1	50	6	5	64
22.5	1.35	50	67	3	50	11	6	69
44.6	1.65	50	100	2	50	36	1	94
			$S_0 = 58''$					
			Observer *W*					
4.4	0.64	50	97″	2″	50	47″	6″	99″
6.4	0.81	50	82	4	50	29	6	81
8.8	0.94	50	67	5	50	14	6	66
17.2	1.24	50	52	4	50	0	5	52
22.5	1.35	50	67	4	50	15	5	67
32.8	1.52	50	85	5	50	16	7	68
44.6	1.65	50	99	1	50	42	8	94
			$S_0 = 52''$					

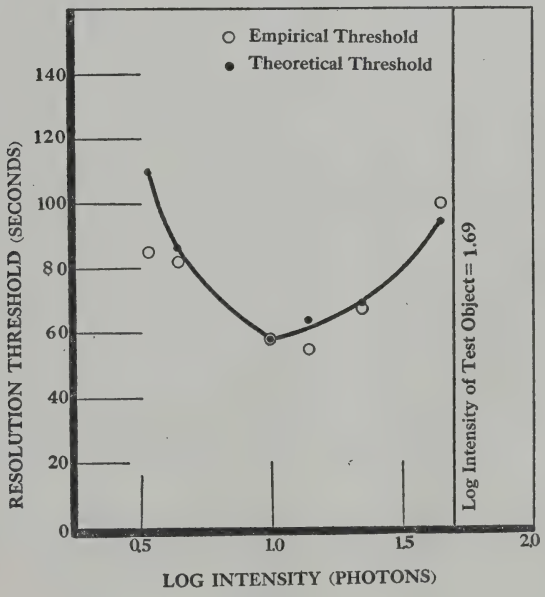

FIGURE 9

SERIES *F* SHOWING THE VARIATION IN THE RESOLUTION THRESHOLD WITH THE TEST OBJECT INTENSITY CONSTANT AT 49.0 PHOTONS, GROUND INTENSITY VARIED

(Observer *C*)

in intensity. It then decreases to a certain minimum after which it rises again. This is similar to the variation in the resolution threshold in *Series A* (illuminated bars on dark ground) and, like *Series A*, is open to the same explanation. Irradiation first decreases to a zero value and then increases with the increasing difference in intensity. S_0 was found to have a value of 58″. The theoretical resolution threshold, calculated from the d measurements, shows a close correlation with the empirical results.

Theoretical Discussion

Thus far we have been concerned with establishing an empirical correlation between visual acuity and irradiation, as a basis for interpreting the dependence of acuity upon intensity of light. In the present section we shall inquire into the physiological interpretation of our data. Such an interpretation, at the present time, must involve a certain element of speculation. It should be emphasized, however, that the validity of our argument up to this point rests upon empirical grounds rather than upon the grounds of special physiological hypotheses.

Irradiation is a familiar phenomenon of physiological optics. Thus far we have used irradiation as it refers to a subjective rather than a physical effect, namely, an apparent displacement of seen contours which accompanies changes in light intensity. In its purely physical sense irradiation denotes a spreading of light on the retina due to the defects of the image-forming mechanism of the eye. It is to irradiation in the purely physical sense that we now turn as offering the general background for our discussion of the problem of the relationship between the resolution threshold and intensity.

On account of chromatic aberration and other sources of optical error the eye does not form the perfect images which would be calculated from the principles of geometrical optics.[5] Let us illustrate by means of a diagram. In Figure 10, horizontal distances represent distances on the retina, and vertical distances represent intensities of light in the retinal image. A represents a point on the border line between two fields of different brightness. If the retinal

[5]Calculations made by Creed (2) on the dioptric errors of the normal eye indicate that "images of easily discriminable objects overlap one another to such an extent that the existence of unexcited cones between them is incredible."

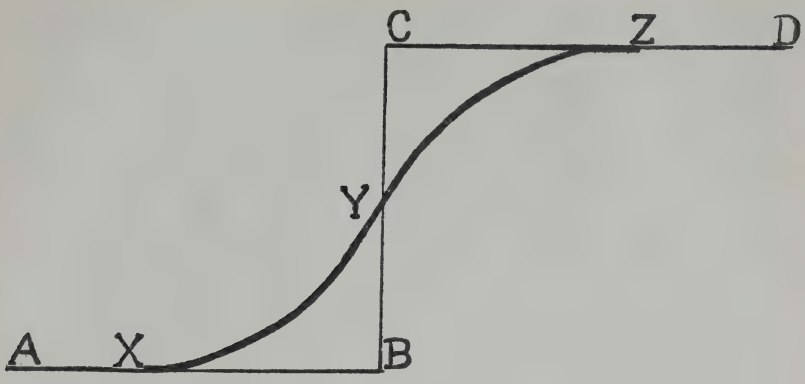

FIGURE 10
BASE LINE INDICATES ZERO INTENSITY

image were ideally sharp, the distribution of light in the image would be represented by the line *ABCD*. The actual distribution, however, corresponds to the line *XYZ* with a gradual rather than an abrupt gradient.

The line *XYZ* is a cross-section of what Mach and Hering call the "light-surface" (Lichtfläche). If our perceptions exactly reproduced the light-surface on the retina, all objects would appear to us with extremely blurred contours. Our vision is actually, however, much sharper than the form of the light-surface would suggest. Certain physiological factors must intervene between stimulus and sensation to produce a "sensation-surface" (Empfindungsfläche) which is a close approximation of the form *ABCD*.

In the retinal image, there is no genuine contour between the bright and dark areas, but only a gradual and continuous gradient of intensity. In perception, on the contrary, there is ordinarily a fairly sharp contour line, separating two regions each of which appears homogeneous in intensity. Two questions may be asked: *First, why do we see a sharp contour rather than a steady gradient? Second,*

where is this subjective contour located with respect to the line BC (the ideal contour of geometrical optics)?

It is a well-known fact that the position of such subjective contours is not always constant, but varies with the intensity conditions, as well as with other factors, into which we shall not attempt to enter here.[6] Our irradiation measurements show how these contours may be displaced by changing intensity; and it is with the interpretation of these changes that we are now concerned.

One factor which will play a part in determining the form of the *Empfindungsfläche* is the existence of a finite threshold for brightness discrimination. This was emphasized by Helmholtz (6, p. 167 ff.). At a certain distance to the left of *BC* and again at a certain distance to the right of *BC,* the difference between the intensity represented by *XYZ* and that represented by *ABCD* will be too small to be perceptible. Thus, only a part of the total optical spreading will be significant for perception. Moreover, the difference threshold itself depends on intensity, in rough accordance with Weber's law. If *CD* is a sufficiently high intensity, *YZ* will be sensibly equivalent to *CD;* the diminution of intensity to the right of *BC* will be below the threshold. If *AB* is a relatively low intensity, a very small addition to it will be perceptible; thus a certain portion of *XY* will be sensed as brighter than the surrounding level *AB.*

Under these conditions, we should expect the subjective contour to appear not at *Y,* but at some place to the left of *Y.* Furthermore, if we increase the intensity level, *CD,* the apparent contour should move still farther to the left of *Y,* since a still greater part of *XY* will be sensibly brighter than *AB.*

This reasoning provides an explanation for the apparent increase in size of a very bright object on a dark background, as well as the apparent decrease in size of a dark object on a very bright background. Our irradiation measurements in *Series A* and *B,* at the higher intensity levels, illustrate this effect.

We also found (*Series C* and *F*) that the apparent irradiation of a very bright object on a relatively dark ground could be *decreased* by adding light to its background. Obviously, an increase of the level *AB* will cause a smaller part of *XY* to stand out as brighter

[6] See Hofmann (8, p. 1 ff.) for a discussion of these factors.

than the adjoining level *AB*, and will therefore cause the apparent contour to move towards *Y*.

Thus the irradiation phenomenon occurring at high intensities (of object or background) can be readily interpreted in terms of very simple concepts. But the factors thus far mentioned do not suffice to explain all the phenomena. Take, for example, the case of a bright object on a completely dark ground (*Series A*). In this case we found irradiation to be large not only at high intensities, but also at *low* intensities. Moreover, not only in *Series A*, but also in *Series B* (dark test object on a bright ground) we found the quantity *d* to be positive; i.e., the bars of the test object appeared larger than they should according to geometrical optics.

We have to consider, then, the more general case in which the intensity difference between the levels *CD* and *AB* is not extremely large. In this case the slope of *XYZ* will be very gradual indeed. Nevertheless, unless the intensity difference is very small (on the order of the difference threshold) we still see a rather sharp contour line.[7] In other words, the *Empfindungsfläche* approximates the ideal form *ABCD* rather than the retinal image curve *XYZ*. At the same time, however, the subjective contour does not, in general, coincide with the ideal contour *BC*. Under some conditions it appears between *X* and *Y*, and under other conditions between *Y* and *Z*. We have to return, then, to the two questions which were proposed above, namely: (*a*) Why does the contour appear sharp? and (*b*) Where does the contour appear under a given set of intensity conditions?

Up to this point, we have left out of account an important fact, which, it seems certain, must be included in any adequate theory of contour perception. We cannot consider the physiological response to the retinal pattern as consisting of so many independent local excitations. As Mach and Hering emphasized, the influence of physiological interaction of excitations, in the form of *simultaneous* contrast, must be allowed for. Where bright and dark areas adjoin on the retina, contrast causes the bright areas to be still further brightened, and the dark areas to be blackened (7). If we apply this conception to Figure 10 we see that it would imply a correction of the *Empfindungsfläche* in the direction of an approximation of the ideal form *ABCD*. It is as if the ordinates between *X* and *Y*

[7]This was verified by both observers in the experiments.

of *XYZ* were lowered, and those between *Y* and *Z* were raised. The steepness of the gradient will be effectively increased and, if the contrast effect is sufficiently strong, there will be a clear-cut subjective boundary between the bright and dark surfaces rather than a hazy gradient of brilliance.

The higher the intensity of the brighter region, the stronger will be its inhibitory or darkening effect on the contiguous region of dim haze. If we consider the effect of contrast in isolation from that of other factors acting, we should expect an increase of the intensity of a bright object on a dark ground to *diminish* its apparent size. Taking the converse case of a dark object on a bright ground, we should expect that raising the intensity of the background would *increase* the apparent size of the object. In either case, the subjective contour should move in the direction away from *X* and towards *Y* (Figure 10).

However, we cannot consider contrast as the only important factor. We have already mentioned another factor which undoubtedly plays an important part in the phenomenon, namely, the threshold for brightness discrimination. If we consider the phenomenon from the point of view of this factor alone, then, as we have already seen, we should expect an increase of the intensity of a bright object on a dark ground to increase its *apparent* size. A dark object on a bright ground, on the other hand, should be *decreased* in size when the background is made brighter. Thus, the contrast factor and the threshold factor would work in opposite directions.

Now, let us see how well our actual measurements fit in with the hypothesis that the *changes of apparent size with intensity are the resultant effect of changes in the influence of these two factors.* This hypothesis is essentially that proposed by Ewald Hering in his discussion of irradiation phenomena (7).

Let us take first *Series A* (test object illuminated, ground dark). Here we find apparent size large at low intensities. The apparent size then decreases progressively to a minimum as the intensity is raised, and finally increases again at the highest intensities. The course of these changes can be accounted for in a general way on the basis of the Hering hypothesis. As intensity is raised, apparent size is at first decreased, because of the greater contrast effect; but the threshold factor, which has the opposite tendency, cuts across the contrast factor and eventually becomes the stronger of the two, so that at the highest levels we have an increase of apparent size with intensity.

Let us consider next the data of *Series B* (dark test object on illuminated ground). In this case, the apparent size of the dark object *decreases* with intensity throughout the entire range. There is not a preliminary stage of increase in size with intensity in the lower range, as we might predict on the basis of the contrast factor.

Do these results agree with the Hering hypothesis? It seems that they may, if we take into account the following consideration. A bright region, as we have said, exerts an inhibitory or blackening effect on a neighboring dark region, and this inhibition is increased by raising the intensity of the bright area. But our experience with contrast phenomena shows that this blackening cannot be increased indefinitely by increasing the intensity of the surroundings. The contrast effect seems rather to tend asymptotically toward a certain maximum value, beyond which it cannot be further increased. Now, in the case of our narrow black bars which are completely surrounded by a bright field, the contrast effect must be very strong, in other words, relatively near its absolute maximum even when the intensity of the ground is low. If this is the case, an increase of intensity will cause relatively little increase in the strength of contrast. The net effect then, due to the action of the threshold factor, might be a decrease of apparent size of the black bars and this decrease would simply become progressively greater throughout the intensity range.

We next consider the more general problem found in all our irradiation measurements. The bars of the test object always appear abnormally large—or, the quantity d is positive—whenever the intensity difference between the test object and its background is very small. This is not the case for faintly illuminated bars on a completely dark background only (*Series A*), but is also true in the converse case of completely dark bars on a faintly illuminated ground (*Series B*). The same phenomenon is observable when the intensity difference is very slight between illuminated test object and illuminated ground (*Series C-F*). In other words, when the difference between the test object and ground is slight, no matter which of the two is the brighter, we find an apparent irradiation outward from the center of the test object and into its background. Perhaps we may say that the small object under these conditions assimilates a larger part of the curve XYZ to its own area.

The irradiation hypothesis does not assume an "irradiation" in the sense of a physiological irradiation in the nervous system, such as

was assumed by Plateau. It assumes a physical spreading of light in the eye; this spreading is physically constant but the degree to which it is effective physiologically and psychologically may vary due to the existence of the threshold and of simultaneous contrast.

It is possible, however, that a physiological irradiation such as described by Plateau (6, p. 400) may exist *in addition to the physical one.* Recently Graham (4) has advanced arguments based on the experimental evidence of Beitel (1) and Karn (9) and others in favor of such a diffusion. Certainly, the phenomenon already described as occurring whenever the intensity difference between the test object and its background is very small is suggestive of such a diffusion.

If such a physiological irradiation exists, it may take place in the case of black objects as well as in the case of white. For we have reason to believe that the black sensation rests upon definite physiological excitation.

Experiments show that the physiological irradiation or diffusion is greater for two stimuli than for either alone. We may assume that, in the case of the two bars, the area of diffusion extends beyond the limits of the retinal image so that the bars appear enlarged. This interpretation is in accord with the results of the irradiation measurements.

Summary

1. The dependence of the resolution threshold (least perceptible interval in terms of visual angle) upon light intensity was investigated by altering the intensity conditions in a number of fundamental aspects. The test object consisted of two parallel bars (width, 2′ 22″; height, 20′) on a background (width, 70′; height, 20′). Stimulation was limited to the foveal area.

2. The main results of the experiments on the resolution threshold may be summarized as follows: Increasing the illumination of the test object with background light free first lowered and then raised the threshold, with a distinct minimum value at medium intensities. With the test object light free, ground illuminated, the threshold decreased steadily with intensity, the rate of decrease becoming progressively less, and producing the relationship usually said to hold between the threshold and intensity. In other series of experiments, both test object and ground were illuminated. When

the test object was brighter than the ground, the threshold decreased to a minimum value and then increased with an extreme difference in intensity; in the converse case, with the ground brighter than the test object, the threshold decreased with the difference in intensity.

3. During the course of the experiments, it was noticed that the bars although physically constant in size appeared to change appreciably in width, and that this change in apparent width acted to raise or lower the threshold of resolution by making the bars appear to expand and decrease the interspace or contract and increase the interspace.

In order to test this hypothesis, an equation was established between the apparent widths of the two bars and their interspace. At some particular intensity, (I_0), the apparent width of the bars appeared equal to the interspace and was physically equal, so that irradiation was neither positive nor negative. The threshold found at I_0 may be taken as a standard and would be found at any other intensity were it not for changes in the apparent widths of the bars. We can, therefore, add or subtract the shift in the position of the inner contours of the test object in order to obtain the theoretical threshold at any intensity other than I_0. Such theoretical thresholds have been calculated from the irradiation measurements and in all cases show satisfactory agreement with the empirical resolution thresholds obtained under comparable intensity conditions.

4. Genuine contours are not found in the retinal image because of the imperfections of the eye; in perception, on the contrary, there is usually a fairly sharp contour between bright and dark areas. Our irradiation measurements show how these contours shift with light intensity.

The spread of light in the eye is physically constant but the degree to which it is effective may vary because of the light threshold and contrast. The threshold factor would increase the apparent size of a bright object on a dark ground and diminish the apparent size of a dark object on a bright ground. The contrast factor, on the other hand, would diminish the size of a bright object on a dark ground and increase the size of a dark object on a bright ground. Thus, the threshold and contrast factors work in opposite directions. The dominance of either factor will depend on the response of the eye to the gradient of intensity in the retinal image. The positive irradiation, found at low intensities, in which the apparent width

of the bars is always increased no matter whether test object or ground is the more highly illuminated, is attributed to summation, or the interaction of retinal areas in lowering the light threshold. A theoretical analysis is presented of each factor, the resultant effect of which is in accord with the irradiation measurements.

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UNE INTERPRÉTATION DE LA RELATION ENTRE L'ACUITÉ VISUELLE ET L'INTENSITÉ DE LA LUMIÈRE

(Résumé)

I. On peut résumer de la manière suivante les expériences sur la relation du seuil de résolution à l'intensité de la lumière. L'augmentation de l'intensité de l'objet de test, deux barres parallèles, a d'abord baissé et ensuite levé le seuil de résolution. L'augmentation de l'intensité du fond a baissé le seuil, la vitesse de la diminution devenant progressivement plus petite. Quand l'objet de test et le fond ont été tous deux illuminés et l'objet de test a été plus clair que le fond, l'augmentation de la différence d'intensité a d'abord baissé et ensuite levé le seuil. Quand le fond a été le plus clair, l'augmentation de la différence d'intensité a baissé le seuil, la vitesse de la diminution devenant progressivement plus petite.

II. La dépendance du seuil de résolution de l'intensité de la lumière s'explique en termes de l'irradiation ou changement apparent des contours de l'objet de test. On a mesuré la quantité d'irradiation, et d'après ces mesures, on a calculé des seuils théoriques de résolution qui s'accordent d'une manière satisfaisante avec les seuils empiriques dans des conditions comparables de l'intensité. On présente une analyse théorique des phénomènes de l'irradiation.

WILCOX

EINE DEUTUNG DES VERHÄLTNISSES ZWISCHEN GESICHTSSCHÄRFE UND LICHTINTENSITÄT

(Referat)

I. Die Experimente über das Verhältnis der Auflösungsschwelle und der Lichtintensität können folgenderweise zusammengefasst werden. Durch die Vermehrung der Intensität von zwei Parallelstangen wurde die Schwelle der Auflösung zuerst gehoben und dann heruntergebracht. Durch die Vermehrung der Intensität des Grundes liess sich die Schwelle sinken, indem die Abnahme fortschreitend weniger wurde. Wenn sowohl das Objekt als auch der Grund beleuchtet wurden, und das objekt heller als der Grund war, liess die Vermehrung des Intensitätsunterschiedes die Schwelle zuerst sinken und dann erhob sie. Wenn der Grund heller war, wurde durch die Vermehrung des Intensitätsunterschiedes die Schwelle heruntergebracht, indem die Schnelligkeit der Senkung fortschreitend weniger wurde.

II. Die Abhängigkeit der Auflösungsschwelle von der Lichtintensität wird in bezug auf Irradiation oder Scheinverschiebung der Umrisse des objekts erklärt. Die Menge der Irradiation wurde gemessen und von diesen Messungen wurden theoretische Auflösungsschwellen berechnet, die befriedigende Übereinstimmung mit den empirischen Schwellen unter vergleichbaren Intensitätsumständen aufweisen. Eine theoretische Analyse der Irradiationserscheinungen wird angegeben.

WILCOX

EIDETIC IMAGERY AND PLASTICITY OF PERCEPTION*

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D. M. PURDY

THE CONTRIBUTIONS OF JAENSCH

Following Erich Jaensch, one describes mental imagery as eidetic when it possesses the full vividness of actual perception, and when it is very definitely localized in perceptual space. The visual kind of eidetic imagery seems to be commoner, and has been more thoroughly studied, than any other kind, although the occurrence of auditory, olfactory, tactual, and motor eidetic imagery has also been reported.

According to Jaensch, persons of the eidetic type differ from other persons not only in their imagery but in their perceptions as well. The visual world of the *Eidetiker* is fluid and plastic, showing a decided lack of strict correspondence with objective stimuli. For example, the apparent size or shape of an object shows a pronounced dependence upon many other factors than the size or shape of the retinal image. Objects often appear to move even when the stimulus is stationary. Thus some *Eidetiker,* when they look at a picture or a statuette portraying human figures in motion, may experience genuine phenomenal movements of these figures (3, p. 150; 4, p. 145 ff.). In one experiment, the subjects (eidetic children) were shown two real objects, an apple and a cane with a curved handle. It is reported that several of them were able "optically to seize the apple with the cane," that is, they saw the cane displace itself towards the apple and then pull the apple by means of its crook (2, pt. 1, p. 200). Not all *Eidetiker* experience such pronounced phenomena; but all of them, if Jaensch is right, have more plastic perceptions than the non-eidetic person.

The eidetic subject is characterized by Jaensch as one in whom the two mental functions of perception and imagery are not sharply

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differentiated from each other. In the non-eidetic person these functions are differentiated in two important ways: on the one hand, images lack the vividness and "bodilyness" which perceptions have; on the other hand, perceptions are comparatively rigid and stable, lacking the modifiability or plasticity which belongs to images. In the *Eidetiker,* images have a high degree of vividness, and perceptions have a high degree of plasticity. With these persons, therefore, it is difficult to draw a sharp line between perception and imagery.

Jaensch finds eidetic imagery more frequent, and perceptual plasticity more pronounced in children than in adults. According to him, increasing age tends to bring about an increasing differentiation between the two functions of perception and imagery. However, individuals are unlike *ab initio* in the degree of this differentiation.

Furthermore, this lack of separation, or, as Jaensch calls it, *integration* of the two functions, is, upon his theory, only one symptom of a more general property of integration of the total psychophysical organism. Eidetic imagery and perceptual plasticity supply a paradigm for integration and a convenient index to assess its degree.[1] But the concept of integration extends far beyond the field of perception and imagery.

Integration means "the mutual interpenetration of psychical functions." Here we may usefully quote the concise résumé of H. Rohracher.

> When the various mental activities work *with* each other, and *within* each other, i.e., when in a single experience thoughts, feelings, will-impulses, ideas, etc., strongly coöperate instead of being individualized and separated, there is strong integration. Such a coöperation of mental processes is by no means a matter of course. There are all possible degrees of this integration, and the extremes represent very different kinds of human beings. On the one hand there are men who have "pure" and "isolated" experiences, i.e., who either *merely* think or *merely* feel or *merely* imagine (of course the "purity" and "isolation" of such processes is never entirely complete), and on the other hand there are men to whom *mere* thought or feeling or will is unknown. In men of this latter sort every individual experience contains a wealth of other mental processes which

[1]Jaensch evidently does not regard eidetic imagery as an absolute index, for he says that "many, indeed most of the individuals to whom the concept of integration applies never have eidetic phenomena" (3, p. 14).

> influence the main process, contribute a distraction to it, supply it with new contents, and so on (6, p. 53 f.).

Between "integrate" and "disintegrate" human beings there is somewhat the same *kind* of contrast as between "organic" and "inorganic" systems. The nervous system of the integrate is of the "vegetative" type, i.e., it is characterized by a strong interaction of individual processes. The nervous system of the disintegrate is of the "cerebro-spinal" type, i.e., in it the interaction of processes is relatively weak. The integrate type is a "youthful" type, the disintegrate a type of maturity. Women tend to be more highly integrated than men.

Pronounced integrates have a strong "coherence" with their external world; in their experience the separation between world and ego is less emphatic than in that of disintegrates. The disintegrate "has little contact with the surrounding world; he experiences everything coming from outside as foreign and as if separated from him by a wall" (6, p. 55).

Integrates are more personal and emotional in their reactions, disintegrates more impersonal and cold. Integrates are less likely than disintegrates to accept and efficiently to perform stereotyped tasks which are meaningless in relation to the needs of the total personality. The artist furnishes a paradigm for integration ("integrate experience is the alphabet of art"), the American business man for disintegration.

Modern civilization is said to have a disintegrative effect upon personalities; thus disintegrates are especially common in America. Another important environmental factor is sunlight, which increases integration; hence people in southern climates tend to be more strongly integrated than those in northern climates. Furthermore, people of Mediterranean race tend to have higher integration than those of Nordic race.

Jaensch not only distinguishes between integrates and disintegrates, but he describes several different *kinds* of integrate personalities. The I_1 type is "generally and outwardly integrated"; this type is characterized by an especially strong coherence between self and world. Persons of the I_2 type are "conditionally and temporarily integrated." Their integration is dominated by certain "inner complexes" which react to outer stimuli that harmonize with them. The I_3 type is "inwardly integrated"; persons of this type lack strong

coherence with the outer world, but the functions of their "inner life" are closely integrated with one another. The synaesthetic or *S* type has, like I_1, a strong coherence with the external world. In I_1, however, the external objects play a dominating rôle in this coherence; the individual's inner life is shaped by external reality. With *S*, the subject dominates over the object; the outer world of experience readily undergoes modification in the interests of the inner world. The synaesthetic type is so named because, as Jaensch states, persons of this type display "colored hearing" or some other form of synaesthesia.

Individuals having "exaggerated integration" (*übersteigerte Integration*) are classified under the basedovoid or *B* type. Jaensch chooses this name because, as he maintains, such persons tend to show the physical symptoms of Basedow's (Graves's) disease, especially enlarged thyroid gland and prominent eyes. The *B* type comprises very strongly integrated persons who belong to the I_1 group or who are intermediate between I_1 and *S*.

Jaensch has also spoken of a tetanoid or *T* type. He now seems to regard this type as pathological and as of comparatively small importance for the theory of the normal personality (1, p. 99 f.).

It is of course impossible to give an adequate summary, in a short space, of Jaensch's very bold and ingenious ideas regarding perception, imagery, and the organization of personality. The ultimate outcome of these ideas still remains very uncertain. It must be acknowledged, however, that he has had the originality and insight to envisage and attack some of the central problems of psychology, and to propose many hypotheses which lend themselves to empirical test.

Jaensch's theories have scarcely received a degree of attention that is commensurate with their far-reaching claims. This is doubtless largely due to the fact that these claims run so far beyond established truth, and that Jaensch rarely makes a very clear distinction between established truth and hypothesis. If one confines one's attention to Jaensch's empirical results, one often finds them difficult to evaluate. General conclusions are stated without any accompanying proof that would satisfy the usual canons of science; and the statement of these conclusions is frequently intermingled with bewildering speculations about epistemology, metaphysics, photobiology, parapsychology, and Fascism.

It seems highly desirable that Jaensch's more important results be tested by independent investigators. So far such checks have been comparatively few. The present paper represents a small contribution of this sort.

This paper reports a study of a single eidetic subject. This person not only has very unusual eidetic capacity but also furnishes some striking examples of "plasticity of perception." The case is one which, upon Jaensch's ideas, would apparently be classified as a specimen of "exaggerated integration." Since Jaensch has devoted much attention to individuals of this sort, our case furnishes interesting opportunities for a comparison of findings.

Unfortunately our subject was available only for a brief time, and our investigation is very incomplete. This study deals mainly with the imagery and perception of the subject, but it has some interest from the point of view of Jaensch's theory of the total personality.

Eidetic Imagery of the Subject *R*

The subject, *R*, is a woman; at the time of our investigation she was a university senior, aged 21. She has possessed eidetic imagery as long as she can remember, although she had never suspected that such imagery was at all unusual. Between the ages of 10 and 11½ she was confined to a hospital bed; during this time she often entertained herself by dressing dolls in imaginary garments.

R has very strong eidetic imagery in the visual, auditory, olfactory, and tactual fields. We begin with the visual. If *R* is asked to imagine the appearance of the sun's disk, she reports a glaringly bright image which refuses to disappear for several minutes. She declares that this image causes her eyes to water and to smart.[2] As the image fades it turns into a purple disk with a white rim—an appearance which is suggestive of the after-images that one obtains from the actual sun.

After looking at pictures *R* often obtains vivid images which enable her to report a great amount of detail with accuracy. She obtains the best images from pictures that are meaningful and interesting to her. (This was also characteristic of many of Jaensch's

[2]Jaensch and Köhler (3, p. 479 ff.) report that brilliant eidetic images may be accompanied by a pronounced contraction of the pupils.

eidetic subjects.) It is comparatively difficult for her to obtain eidetic images from nonsense figures. Colored pictures give better eidetic images than black and white pictures.

The eidetic images are often modified with respect to the original. (This was also typical for many of Jaensch's observers.) Thus, a rather "impressionistic" painting of a landscape is transformed, in the eidetic image, into a more "naturalistic" picture. If the picture contains such objects as rivers or trees, these objects are likely to display movement in the eidetic image. The image has a thoroughly realistic three-dimensional appearance, far more vivid than that of the original picture.[3]

The colors of the image are like those of the original, but they are much more saturated. They usually have a glossy, theatrical appearance; *R* says they resemble pictures seen in a stereoscope.[4] This description tallies with those made by Jaensch's subjects.

Jaensch usually had his subjects project the image upon a screen, such as a sheet of gray cardboard. *R* declares that, in her case, the presence or absence of such a screen is a matter of indifference. She can place the image at any point she likes—e.g., at a point "in the air" in the middle of a room. In such a case the image ordinarily appears transparent, and the real surroundings are seen through it. Sometimes, however, the real surroundings are suppressed by the image and disappear completely (*vide infra*).

In the eidetic image of a black and white silhouette, the original blacks are often replaced by whites, and the whites by blacks, as in a negative after-image. A similar phenomenon was found by Klüver (5, p. 105) in some of his eidetic subjects.

When *R* projects an eidetic image upon a screen, the phenomenal size of the image increases with the distance of the screen. Thus, the image of a small match-box appeared about 1 cm. long when projected on a sheet of paper at 15 cm. distance, while it appeared about 2 meters long when projected on the wall of a building some

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[3]Cf. Jaensch: "A distinguished scholar who possesses strong eidetic images once told me that he has always been secretly amused to see people look at stereoscopic pictures, because he himself can obtain from any simple photograph or post-card a picture that is like reality in solidity and in size as well, and the illusion-value of this picture is not increased by using 'that ridiculous apparatus'" (2, pt. 1, p. 228).

[4]One of Jaensch's observers described his eidetic images as "shapes of frozen gas."

50 meters away. An increase of apparent size with distance is said by Jaensch to be typical of eidetic images.

When *R* views an object and then projects its eidetic image on a screen at the same distance, the image usually appears larger than the object itself.

However, there is no absolutely fixed relationship between the apparent size of the image and its apparent distance, since *R* can voluntarily cause the image to grow or shrink.

For *R*, as for Jaensch's subjects, the images usually follow the movements of the eyes. This is not always the case, however. For example, if *R* forms an eidetic image of a group of human figures containing, say, 8 or 10 members, this image remains stationary when the eyes are moved—while an image of a single person moves with the eyes. It is interesting to note that the subjects of Jaensch and Reich also observed that the eidetic images of groups of objects —as contrasted with single objects—often refused to follow the eyes' movements. Jaensch and Reich also found that the image of an apparently "heavy" or "difficultly movable" object tended to remain motionless (2, pt. 1, p. 55).

With *R*, as with Jaensch's subjects, an eidetic image tilts when the head is tilted, and in the same direction. According to *R*'s reports, not all images tilt equally much; thus, for the same inclination of the head, the image of a human figure tilts less than the image of a small ink-line.

After *R* has inspected an object, and the object is removed, the eidetic image ordinarily does not appear at once, but only after several seconds have elapsed. If the object is complex, like a picture containing many details, the parts develop successively, rather than simultaneously, in the eidetic image.

When *R* inspects an eidetic image, she moves her eyes about from one part to another, just as she would do in examining a real object. She declares that a detail can be seen distinctly only when she is "looking at it." She often has the experience of seeing a vague detail "out of the corner of her eye," and then making it become distinct by "fixating" it. This extremely interesting peculiarity of eidetic images has evidently been noted by Jaensch (cf. 2, pt. 1, p. 54), although apparently never very much emphasized by him.

Another interesting observation is the following. On one occasion *R* was shown a picture of a table covered with many dishes of food,

and then asked to describe the details on the basis of her eidetic image. She reported a great variety of details accurately, but failed to identify one object, a dish of fruit in a remote corner of the table. At the corresponding place in the image she saw only "a vague cloud-like thing that seemed to be waving back and forth." I made the suggestion: "Could it be a fruit dish?" Immediately, according to *R,* the dish and its contents developed out of the "cloud" and assumed very sharp and definite form; and she proceeded to describe the shape and color of the dish, as well as the various pieces of fruit contained in it, with considerable accuracy.

R's eidetic images often undergo very marked changes as she continues to observe them, e.g., when she had been shown a picture of a dog holding a football in its mouth, and had formed an image of this picture, she reported after a time that "the dog had dropped the football." Her eidetic images usually fade away after a few minutes even when she makes an effort to preserve them. Details which are interesting to her tend to persist longer than uninteresting details.

For *R,* the world of actual perception is readily annihilated, as a whole or in part, and replaced by eidetic imagery. Thus, she can abolish the perception of a person who is standing before her open eyes, and in his place see an eidetic vision of some absent person. Through eidetic imagery she can transport herself to some remembered or fantastic scene—a ballroom in Quebec or an island in the South Seas; her real surroundings are sometimes completely blotted out, and sometimes she sees only the marginal part of her "real" visual field. Some of Jaensch's subjects have described similar experiences (cf. 2, pt. 1, p. 17 f.; 3, p. 165 ff.).

R can often "remove" particular real objects from her field of vision; for instance, it is easy for her to see a human being as devoid of a head. She can also add many kinds of eidetic details to the things in the real world, e.g., she can place green leaves upon barren winter trees, or supply a smooth-shaven man with a full beard.

Eidetic imagery often appears when *R* does not expect it. Once when she was in a lecture hall filled with students, she suddenly, and for no apparent reason, acquired the impression that all the students were wearing black goggles over their eyes. Usually this spontaneous imagery is more or less congruous with its setting. When she is asked to equip a young man with an imaginary silk hat, not

only does the hat appear but the clothes he is wearing spontaneously change into evening clothes. When she attempts to replace a young lady's coiffure with long ringlets, she sees the young lady as holding a stick of striped candy in her mouth.

R seldom mistakes her eidetic images for reality, but this does happen occasionally. Thus, when riding in an automobile, she has sometimes warned the driver against objects in the road which, as she soon discovered, were figments of her eidetic vision. [Cf. the analogous experiences reported by the subjects of Jaensch and Neuhaus (3, p. 117 ff.).][5]

How does *R* discriminate spontaneous eidetic images from real things? Here the following factors would seem to be important. In the first place, the image often represents something improbable or impossible in the given situation. In the second place, the image shows peculiarities which perceptions do not have. Its color is richer than that of a real object, and it often has a glassy, spectral appearance. Its size is often abnormal. It usually moves when the eyes move. Also, it ultimately vanishes.

R's eidetic imagery in the non-visual sense departments is also strong. Imagined sounds are really "heard" just as imagined sights are really "seen." If *R* imagines the roar of the sea, the auditory impression is so intense as to weaken actual sounds—e.g., those of voices—in her environment. Sometimes, she declares, these real sounds are completely blotted out. Eidetic images of odors have comparable vividness for *R*. Tactual images are also present; when *R* imagines that an insect is crawling upon her skin, she reports an image that is as intense and irritating as the feeling of a real insect. The tactual image is relatively less intense in the case of some larger object, such as a weight placed upon the skin. Eidetic images of cold and warmth can be obtained, although they seem to be comparatively faint.

R's visual perception and imagery are often supplemented by auditory and olfactory eidetic phenomena. *R* was once shown a picture portraying a hunter aiming his gun at a rabbit. While gazing at this real picture she reported that she suddenly heard the gun make a loud report (and saw smoke issue from its muzzle). When

[5] As Jaensch has suggested (1, p. 22 f.; 2, pt. 1, p. 227 ff.), the experiences of persons with strong eidetic imagery may well have given rise to various traditions about supernatural phenomena.

R imagines a rose she not only "sees" it but has a vivid consciousness of its odor. When she forms a picture of ocean waves breaking on the shore, she has eidetic imagery of the sound and smell of the sea. When, in one of her fantasies, she imagines herself in the presence of a doctor, he appears to be accompanied by the odor of ether.

Plasticity of Perception

We have already seen how, in *R*'s case, the real world can be supplemented by eidetic elements, and how it can be blotted out, wholly or partly, in favor of eidetic constructions. Some further observations will now be related which show the remarkably loose dependence of *R*'s phenomenal experience upon the physical conditions of stimulation.

R can obtain very vivid impressions of movement from objectively stationary things. These impressions may be voluntarily or involuntarily produced. Looking at a card covered with various small geometrical figures, she sees one figure (a triangle) spontaneously move towards another figure (a square), circle it a few times, and then return to its original place. When she looks at three short parallel lines, no movement takes place until she exerts a deliberate effort to produce it, whereupon the two outer ones move towards the middle one and fuse with it. She finds it easier to produce such a movement when the lines are vertical than when they are horizontal.

By exerting an effort, she can make a human being appear to rise in the air and float across the room. After such an apparent movement has started, she cannot always control its course completely. Thus, when she is asked to make the body float out the window, she reports that it perches itself on the window-sill and refuses to go farther.

At one time she was shown a card having a zigzag line drawn upon it in black ink, which produced an unexpected effect. She declared that the line immediately transformed itself into a green snake with red eyes, which very swiftly wriggled across the card.

In such a case as this last, one is inclined to suspect that the perception of the real object—or, better, the cerebral process set up by it—has been suppressed in favor of an imaginal process, and that the

experienced movement is actually the movement of an image. The following two observations point to such an interpretation.

On one occasion, *R* was shown an inverted page of print (the title-page of Jaensch's book on *Eidetic Imagery and Typological Methods of Investigation*). She succeeded, upon a suggestion, in making the page appear to turn itself through 180° into the upright position. However, she could not *read* any of the printed matter on the page with the exception of the two large words "Eidetic Imagery." (The letters, incidentally, appeared greatly magnified to her.) In place of the publisher's insignia at the bottom of the page (a small black square containing four white letters: "H B Co"), she saw the insignia of another publisher (Grosset and Dunlap). (She recognized meanwhile that this impression was probably in disagreement with reality.)

Since *R* was unable to read the whole page, and since her impression contained such pronounced eidetic distortions, it looks as though the original impression of the page had been suppressed in favor of an eidetic image, this image being the vehicle of the apparent rotation.

At another time, *R* was shown a fountain pen and its cap, the two articles lying in the same straight line with a distance of about 30 cm. between them. For her, the cap spontaneously moved towards the pen, striking it with a sharp click and then making a series of revolutions which attached it to the pen. After she had reported that the two objects appeared to be fastened together, I covered the real cap with a sheet of paper. *R* declared that she continued to see the cap, attached to the pen as before.

These observations suggest that what appears to be a plasticity of perception—in the narrower sense—may really involve a suppression of perception. One then asks: Is this true of all cases of so-called perceptual plasticity?

In one case at least, that of an experiment by Jaensch's student, Freiling, perceptual plasticity in the stricter sense seems to have been verified. This experiment was a study of the apparent movements of objects which, under certain conditions, are produced by movements of the eyes. Many of Freiling's subjects could always clearly and immediately distinguish a "real" perception from an eidetic image; this was especially easy for those subjects whose eidetic images had colors complementary to those of the real object. These

"real" perceptions were found to undergo apparent movement under the given conditions (2, pt. 1, p. 304).

It seems possible, however, that many of the more striking examples of "plasticity" which have been described by Jaensch may really involve a suppression of perception by imagery.

For *R*, things may appear to change in shape even when they are physically constant. Looking at an outline drawing of an oval, she had the unanticipated impression that the oval moved like a rubber band and transformed itself into a square.

R can voluntarily cause objects to change in apparent size. She succeeded in making a small match-box appear about six times its normal size. As the object grew it became transparent. When *R* tried to make the box appear to shrink to a very small size, a curious effect resulted. The box finally appeared about 1/6 the normal size, remaining opaque and not, as before, transparent. However, some printed letters on the box (the trade-mark "Rosebud") refused to shrink, but retained their normal size and therefore projected into empty space at either end of the box. *R* could, if she wished, make these letters appear small, but in this case it was the box which retained its normal size; it was impossible to make both letters and box shrink at once.

Jaensch has used the following example of "plasticity" as a criterion of strong "psychophysical integration." The subject is asked to look at a straight line, and meanwhile his arms are pulled outwards by the experimenter. A strongly integrated subject experiences an apparent increase in the length of the line (1, p. 97; 3, p. 201 ff.). In most of Jaensch's experiments of this sort, he employed a line with outward-pointing feather-heads at either end, resembling one of Müller-Lyer's illusion figures. He declares, however, that the same effect can sometimes be obtained with a simple straight line.

In an experiment with *R*, a simple line 75 mm. long was employed. As she looked at it, her outstretched arms were vigorously pulled (without previous warning). According to her report, the line appeared to expand by about 6 mm. at either end, but, behaving like an elastic object, it returned to its normal length while the pull on her arms was still continuing.

The Question of Personality Type

We shall next consider the question of classifying the subject *R* according to Jaensch's typology of integration. It may be mentioned, to begin with, that *R* was born in Canada of parents who had emigrated from Scotland. Her ancestry is chiefly Scotch, with a small admixture of French. Most of her life has been spent in the city of Quebec. *R* is of medium height and slender build; she has very large dark brown eyes, a fair skin, and dark auburn hair. She seems to be in good health both physically and mentally; especially is there no suggestion of any neurotic tendencies.

R has good intelligence, but is not very studiously inclined, although she did win the rating of an honor student during her final semester in college. Her interests seem to be social rather than intellectual, and she is apparently very well equilibrated with her social environment. She is interested in art, and has done some painting and drawing, although her talents in this field are apparently not remarkable.

Because *R* has such pronounced eidetic phenomena, one might expect her to belong, according to Jaensch's ideas, to the exaggerated integrate or basedovoid (*B*) type of personality. *R* has the prominent eyes which are said to be characteristic of this type. Whether she has any hyperactivity of the thyroid gland is not known; at any rate she has no goiter. In hair color she is unlike most of the Jaensch and Neuhaus' *B*-type subjects, who had black hair and dark eyes (see 3, p. 91). Jaensch speaks of his *B* types as having very vivacious and expressive faces; *R* tends to be more impassive and non-committal in her facial expression.

In strongly integrated persons, according to Jaensch, the various somatic functions are easily affected by mental influences. "When there is a very close interconnection between the psychical and the somatic sphere, such as exists in children and in eidetic subjects in general, the vegetative system responds even to very slight changes in the mental content" (3, p. 512). The following observation reported by *R* is interesting in this connection. When she imagines herself running, or engaged in some other strenuous exercise, she experiences a very forcible pounding of the heart.

Jaensch and Neuhaus (3, p. 95 ff.) characterize all individuals of the *B* type as having a very strong *Emotionalität*. By *Emotionen*

they mean certain "states of the self," such as feelings or moods, which are not motivated by outer circumstances. *Affektabilität* is also said to be pronounced in these persons. *Affekte,* as they use the term, are characterized by the fact that they are incited by an outer cause, and that they discharge themselves suddenly in a physical manner.[6]

A number of the subjects of Jaensch and Neuhaus tended, when annoyed even by a trivial circumstance, to express themselves by smashing furniture or other objects. Our subject *R* admits that she often has such impulses, although she is almost always able to hold them in check. The day before she was questioned she had been suddenly angered about something, and had felt a very strong impulse to dash her fist through a pane of glass. As a small child, she once became enraged at her nurse and hurled a pair of shears at her; on another occasion she became angry at her parents and impulsively cut off all her own hair to spite them. *R* also reports that she has very pronounced "ups and downs" of mood.

It is evident that *R* has some characteristics which correspond to Jaensch's description of the basedovoid type. Time did not permit any more exhaustive study of *R*'s case from this point of view. Now, according to Jaensch, most basedovoids are exaggerated integrates who belong to the I_1 group. He declares, however, that there are some basedovoids who are intermediate between the I_1 type and the *S* or synaesthetic type (2, pt. 2, p. 471). To what sub-type does *R* belong?

In attempting to answer this question by reference to Jaensch's writings, one is forced to realize the meagerness of the empirical evidence for the various types of integration. This is especially the case with the *S* type. This type is said to be characterized on the one hand by certain very general peculiarities in the organization of personality, and on the other hand by the possession of synaesthesia. In the article by Jaensch and Ingeburg Meier, setting forth the evidence for the *S* type as one of the *Grundformen mensch-*

[6]On the other hand, Jaensch and Neuhaus say that *B*-type persons do not necessarily have a strong *Gemütsleben,* and that with many of them this form of experience is very poor. "According to recent investigations this is especially true of the sub-case of the synaesthetics." The word *Gemüt* is not defined, but one assumes that it applies to the more persistent, less explosive states of anger, joy, etc., which are motivated by an external situation.

lichen Seins, one finds extremely brief character-analyses of only nine subjects (3, p. 425 ff.). It can hardly be said, therefore, that Jaensch has given us any grounds for taking the supposed association of personality structure with synaesthesia very seriously.

R displays certain phenomena, already described, that might be classified under the heading of synaesthesia. Thus, the sight of a picture may be accompanied by auditory eidetic images that are suggested by its meaning. Whether Jaensch would regard such phenomena as indicative of the *S* type is not clear. *R* does not have "colored hearing"; but when she listens to music she experiences a succession of eidetic scenes containing human figures, etc., that form an expressive accompaniment. So far as is known, she does not possess any of the simpler, "meaningless" forms of synaesthesia, although time precluded a careful investigation of this point.

Comparing the two types I_1 and *S,* Jaensch says that "the *S* type is connected with I_1 by very gradual transitions. . . . But between the very pronounced cases of I_1 and of *S* there is a wide distance. I_1 is a reception type, *S* a projection type. Correspondingly, the eidetic phenomena of I_1 are always close to reality, while those of *S* are frequently the offspring of a bizarre fantasy. . . . The perceptual structures are especially labile and loose for *S;* . . . perceptual phenomena display a minimum of rigid dependence on retinal space values. . . . The fantastic constructions of I_1 often reproduce something real; they *always* represent something which is at least *possible.* . . . The contents of a pronounced *S*-fantasy almost never belong to reality; at the most they belong to a "possible" world, but they do not always belong even to that. . . . The *S* type mixes the real and the imaginary" (2, pt. 2, p. 471).

On the basis of these statements one is inclined to suppose that Jaensch would classify *R* under the *S* type. And his next statement would make such a classification absolutely necessary.

> . . . The pronounced *Eidetiker* of the *S* type can often see his eidetic phenomena as located in the midst of real things, with his eyes open, while the I_1 type must either close his eyes or use a homogeneous projection field. At the most, the *Eidetiker* of the I_1 type can see his eidetic visions in the midst of real things when the light is dim, . . . while the *Eidetiker* of the pronounced *S* type succeeds in doing this in bright and sharp light (2, pt. 2, p. 471 f.).

Jaensch seems to hold that all exaggerated integrates belong to I_1 or are intermediate between I_1 and S; and he has little to say about the possibility of exaggerated integrates of the pronounced S type. However, in one place Jaensch and Meier say that ". . . the exaggerated form [of the S type] is pathological, and among these exaggerations may belong many clinical pictures which have recently attracted special attention as types of schizophrenia" (3, p. 455). Here it may be repeated that our subject R is quite normal mentally, and certainly she displays no schizophrenic tendencies.

Regarding the S type, Jaensch and Meier say:

> Sometimes there is a pronounced dream life, a waking dream. . . . Space, time and the surrounding world are completely forgotten. . . . Displaying nothing that is truly psychopathic, and—if we except limiting cases—devoid of pathological symptoms, and yet deviating in many ways from the usual norm, these persons are often regarded with a shaking of the head. Although they belong to the integrate type, . . . they differ from the generally and outwardly integrated I_1 type. This type surrenders itself in a more passive way to the things and events of the outer world; . . . the S type *projects,* so to speak, his inner life into the outer world, and sees that world in the light of his inner world (3, p. 452 f.).

In some degree, perhaps, this description would apply to R. She often impresses one as a person who lives, rather self-sufficiently, in an inaccessible world of her own. One of her friends remarked that "she seems to go around in a daze" and that "she is a person whom nobody really knows." R nevertheless is completely at ease in her relations with other persons, and in fact is notable for a decided lack of self-consciousness. Thus, although "introverted" in one sense in which this term is used, she is thoroughly "extraverted" in another.

Summary

This paper describes the case of a woman college student who has very strong (visual, auditory, olfactory, and tactual) eidetic imagery. Her visual imagery shows many characteristics that agree with the findings of Jaensch. In the case of this subject the world of visual perception can easily be suppressed, either wholly or partly, and replaced by eidetic imagery. Her phenomenal experience shows a

remarkably loose dependence upon the physical conditions of stimulation at the moment. This "plasticity of perception" can be attributed, at least in some cases, to a suppression of perception by imagery. The subject shows a number of the characteristics of Jaensch's "basedovoid" type, as well as some of the traits of his "synaesthetic" type.

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L'IMAGERIE EIDÉTIQUE ET LA PLASTICITÉ DE LA PERCEPTION

(Résumé)

Cet article décrit le cas d'une étudiante universitaire qui montre une très forte imagerie eidétique (visuelle, auditive, olfactive et tactile). Son imagerie visuelle montre bien des caractéristiques qui s'accordent avec les résultats de Jaensch. Dans le cas de ce sujet le monde de la perception visuelle peut être facilement supprimé, ou entièrement ou en partie, et remplacé par l'imagerie eidétique. Son expérience phénomènale montre une dépendance remarquablement légère des conditions physiques de la stimulation au moment. Cette "plasticité de la perception" peut être attribuée, au moins dans quelques cas, à une suppression de la perception par l'imagerie. Le sujet montre nombre des caractéristiques du type "basevoïde" de Jaensch, ainsi que quelques-uns des traits de son type "synesthésique."

Purdy

DIE EIDETIK UND DIE PLASTIZITÄT DER WAHRNEHMUNG

(Referat)

Diese Untersuchung beschreibt den Fall einer Universitätsstudentin, die sehr starke (Gesicht, Gehör, und Tast) eidetische Vorstellungen besass. Ihre Gesichtsvorstellungen weisen viele Eigenschaften auf, die mit den Befunden von Jaensch übereinstimmen. Im Fall dieser Frau kann die Welt der Gesichtswahrnehmung leicht unterdrückt, entweder ganz oder teilweise, und durch Anschauungsbilder ersetzt werden. Ihre phänomenale Erfahrung zeigt eine ausserordentlich lose Abhängigkeit von der physikalischen Zuständen der Reizung des Augenblickes auf. Diese "Plastizität der Wahrnehmung" kann wenigstens in einigen Fällen einer Unterdrückung der Wahrnehmung durch Vorstellungen zugeschrieben werden. Die Vp. weist eine Anzahl der Eigenschaften von Jaenschs Basedovoid Typ, sowie einige der Eigenschaften des Synästhetikers auf.

PURDY

SHORT ARTICLES AND NOTES

PSYCHOLOGICAL "ULTIMATES": A NOTE ON PSYCHOLOGICAL "FACT" VERSUS PSYCHOLOGICAL "LAW"[1]

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The distinction between what are termed scientific facts or data on the one hand and scientific laws or principles on the other hand is fairly widely accepted in most scientific disciplines. Without delving too deeply into abstruse metaphysics, we may accept the term "fact" to mean any event, occurrence, or phenomenon in the space-time manifold. A scientific law, on the other hand, represents the statement of a sequence, order, or relation of phenomena which is invariable under the given conditions. In the light of this distinction, it is often apparent that much of the confusion which reigns in psychological circles today is the result of the failure to recognize this distinction, with the resulting chaotic mixture of the general and the particular, and often leading to the seemingly inevitable assertion that in psychology, as in all social science, there can be no fundamental laws.

It is, of course, apparent that tremendous difficulties are involved in the formulation of general psychological principles. In the first place, the behavioral situation is always exceedingly complex, and the reactions are conditioned by such a multitude of subtle and varied causes that it often becomes at least practically impossible to impose all of the strict experimental controls which are necessitated by the rule of the one variable. Moreover, the experiential, biographical, or historical factor is of the utmost importance in psychological experiments, such not being the case in the so-called physical sciences. Thus the physicist is not immediately concerned with whether his given unit mass has come from communistic Russia or Nazi Germany, although this factor is of paramount importance in the case of psychological behavior. Lastly, mention may be made of the fact that it is often difficult for human beings to regard themselves objectively, without prejudice or bias, and although this fact itself is undoubtedly due to stimulational or cultural factors, it nevertheless cannot be ignored.

With all of these difficulties and limitations, however, it seems that certain general formulations would have been reached, which at least would approximate what might be termed principles or laws of psychological behavior. A survey of contemporary textbooks and experimental and theoreti-

[1]The present paper is an elaboration and extension of the point of view previously expressed by the writer for the field of comparative psychology. Cf. Foley (1, 2).

cal literature reveals in most cases, however, the utter lack of general behavioral laws[2] although the results of investigation after investigation are reported. In fact the reader is continually bewildered with a mass of detailed information, dates, and the names of particular investigators, this material more often approximating encyclopedic compilation than an attempt to derive any generalized formulations. Thus it would seem that the proper distinction between fact and law (as well as between fact and theory, as has so often been reiterated) has not been made in most psychological treatises and investigations.

This contention may be illustrated with reference to widely diverse "fields" of psychology. In "animal" psychology, for example, many studies have been conducted to determine what the animal can do, e.g., how many trials are required to learn the maze, how many latches he can manipulate, or how many sticks he can use. Similarly, in "child" or "genetic" psychology, many experiments have attempted to determine a so-called general pattern or rate of behavioral development. In "abnormal" psychology, the attempt is usually made to discover the so-called types or classes of mental disorder. In "social" psychology, surveys are made to investigate the existence of certain types of institutional behavior, such as susceptibility to propaganda, the effect of suggestion, and behavior in particular group situations. In the statistical studies of so-called "mental organization"[3] in the field of "differential" psychology, the attempt has continually been made to determine what "the" pattern of mental organization "is."

Many theories have been expounded with an aim toward the ultimate formulation of general laws or principles, but rarely have attempts been made to test these hypotheses empirically. The point to be emphasized at present, however, is that such general principles, if of reasonable validity, will take care of diverse and seemingly contradictory facts. Thus, for example, the African native may worship the pig and hold it in the highest personal esteem, whereas the orthodox Jew may abhor its filthiness and refuse to eat it. Surely these are diverse facts, and yet the behavior of both the African and Jew can be understood in terms of the same basic formulations.

This primacy of general principles may be illustrated in the case of any of the so-called "topics" of psychology, such as perception, memory, language, emotion, motivation, thought, or creative behavior. All of such

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[2]This criticism cannot be made of the comprehensive attempts at systematic formulation and experiment made by certain psychologists, such as Guthrie, Hollingworth, Hull, Hunter, Kantor, Weiss, and others.

[3]It seems extremely unfortunate that the term "mental organization" has been used to refer to the results of statistical analysis of certain limited materials; and it is also to be regretted that only those who employ such frequently questionable techniques have been credited with data or theories concerning the organization of the mind.

activities are a function of certain historical stimulus-response relationships, and the primary and important thing, so far as the systematic formulation of psychological science is concerned, is that these stimulus-response sequences have occurred. The general principle or psychological "ultimate" will be found in the pattern of these stimulus-response relationships, rather than in an analysis of what the particular response happens to be at any moment. Thus in the case of linguistic behavior, for example, the systematic psychologist is not particularly interested in whether the given individual speaks English, German, or Zulu, but rather in the "reasons why" he speaks it, and there is no evidence whatsoever to discredit the statement that these "reasons why" will be the same regardless of what particular language is spoken. Unfortunately space does not permit the elaboration of this viewpoint and its ready extension to all other forms of behavior. The point is merely that the acquisition of psychological behavior always occurs in conformity to the same general principles,[4] although the particular behavior acquired differs in the most unimaginable fashions.[5]

Thus it would seem that more emphasis should be placed upon how the animal learns rather than upon what it happens to have learned (animal psychology), upon the process by which antecedent factors condition the pattern and rate of behavioral development in the child or infra-human organism (child and genetic psychology), upon the stimulating conditions for particular behavioral deviations (abnormal psychology), upon the institutional stimuli and resulting cultural behavior (social psychology), and upon the genetic or antecedent conditions determining what particular pattern of "mental organization" will be exhibited (differential psychology). It is only through the study of the historical interaction between organism and stimulus objects and events that anything corresponding to general laws or principles can be evolved. The principles of intelligibility thus arrived at, these Aristotelian *"archai"* or "basic truths" of all psychological becoming, will be generally applicable to a wide variety of diverse facts, and in this sense will be unchanging and timeless—at least an approximation to what might be termed psychological or behavioral "ultimates."

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[4]The writer is here merely expressing the need for and advantage of such general laws or principles; a future article will deal more specifically with their formulation.

[5]It will be observed that this theoretical formulation is in strict conformity with the principle of parsimony, since an almost infinite number of behavior facts are envisaged as the function of a minimal number of behavior principles.

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APPARATUS

THE TREMOGRAPH

An Improved and Modified Form of the Luria Apparatus

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More than three years have elapsed since the publication, in English, of Luria's *The Nature of Human Conflicts.* In this book Luria described a new method of investigating affectively toned processes, both conscious and unconscious. Knowledge concerning the action and nature of these processes is of great importance to psychology at the present time. Hundreds of attempts have been made to discover the nature of these processes, especially by use of the psychogalvanic skin response technique. On the whole the results obtained have not been gratifying. Much remains to be done. It is, therefore, astonishing to find that only a few psychologists have made any attempt to employ the new Luria technique in their investigations, despite the fact that, on the face of it, the technique holds much promise.

It is not the purpose of this communication to analyze the lack of interest manifested by American psychologists in this new method of investigation, but to suggest and attempt to remedy one of its probable causes. Luria's original apparatus was unwieldly, inconvenient, and undesirable. It is our belief that these characteristics of the apparatus have had much to do with the lack of interest and unwillingness of psychologists at least to give the method a fair trial.

Luria used two systems of tambours—one for the active hand and one for the passive hand. Each of the systems was composed of two tambours, one for activation by the tips of the fingers and the other for recording these activations on the kymograph. The difficulties of leakage, stretching dams, equalizing tensions, and maintaining constant pressures, etc., are so well known that they need not be repeated here. A further difficulty of the tambour system, as used by Luria, lies in the fact that the two tambours to be activated are placed in one part of the experimental room where the subject is seated, while the recording tambours are located behind a heavy screen where the experimenter observes the recording of the impulses on a kymograph. The experimenter is therefore shut off from the subject and is deprived of an opportunity of observing any external manifestations on the part of the latter, which may serve as corroborating evidence of underlying affective processes.

The kymograph used by Luria was of the smoked paper variety. The general mussiness of the smoking technique, together with the difficulty of

attaching the paper properly to the drum, removing the paper without smooching, and shellacing the record for preservation are favorite topics of conversation in every laboratory, and almost every experimenter that has used them secretly dreads the next time the procedure will be necessary.

These difficulties alone are sufficient to discourage a great many investigators from launching into a new field of research that employs them. A further complication arises when an attempt is made to take accurate measures from such records. Due to the pivoting of the stylus arm the graphs are usually distorted to give a horned effect. Such records are fairly satisfactory for qualitative analysis but are extremely unsatisfactory when employed quantitatively. Moreover, the smoked paper technique definitely limits the duration of a proposed study since new complications arise in the sagging of the smoked paper when a certain length is passed. It has been asserted by one thoroughly acquainted with smoked paper kymographs that the number of complications varies directly as the square of the length of the record.

Attempts have been made to overcome the difficulties of the smoked paper kymograph by using waxed paper instead. Although this is undoubtedly much cleaner and more practical there still remain the difficulties of friction on the paper, wax clogging the styli, failure to record due to defective spots in the waxed surface, and, perhaps most important of all, the difficulty of keeping the paper running straight. The C. H. Stoelting Co., who have found the Luria Technique valuable enough to include in their Darrow Behavior Research Polygraph (September, 1934), have overcome some of these difficulties by recording on bromide paper. They have, however, retained the tambour systems. Since the cost of the Stoelting apparatus is far beyond the reach of most laboratories, and since it is combined with a number of other techniques, there seems to be a real need for a comparatively cheap, convenient, and effective apparatus, free of complications and mussiness, in order that this new field of experimentation may be more adequately explored.

The apparatus to be described was developed to fill this need. In order to avoid the difficulties inherent in the smoked paper technique, or the friction and clogging of styli in the waxed paper technique, photography was resorted to. In an attempt to avoid the various complications of tambour systems a set of light levers was constructed to replace them.

The overall dimensions of our apparatus are 20″ x 20″ x 7″ although these could easily be reduced to about 18″ x 13″ x 6″. From outward appearances it is simply an aluminum box with two finger platforms protruding on the upper surface. The floor of the box is of sheet aluminum ⅛″ thick and rests on four rubber typewriter feet to facilitate handling and to absorb any vibration. Eight brass pillars ⅜″ square are fastened to the floor and these support the roof and sides. The pillars are located in each of the four

corners with one midway between each of these four. The roof is also constructed of ⅛″ sheet aluminum and the sides of 1/16″ aluminum. Both sides and roof are screwed to the pillars in order that any one may easily be removed for purposes of adjustment. Within this small portable box the entire apparatus is contained with the exception of the batteries and the key for controlling the stimulus signal to be described. The wires leading from these external sources are connected to the apparatus on the rear side by means of polarized plugs.

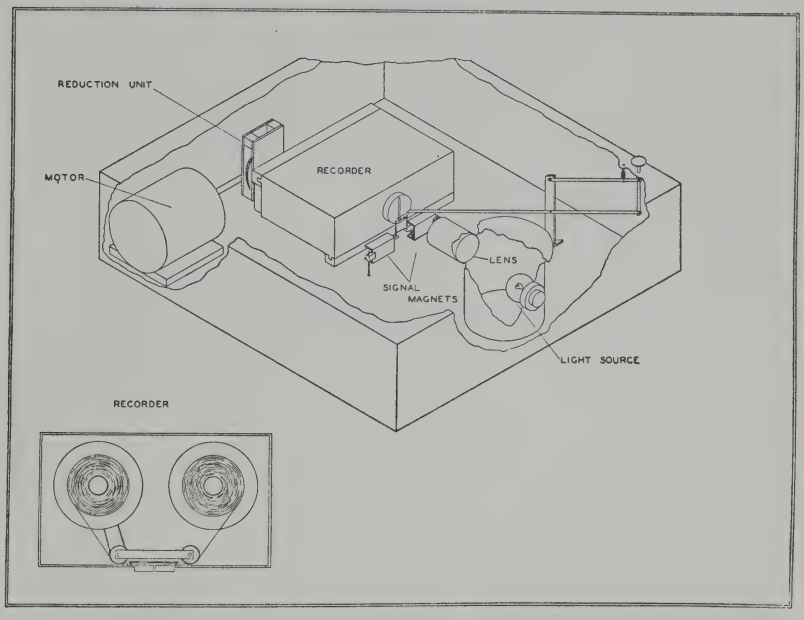

FIGURE 1

THE TREMOGRAPH

Within the box are located a recording box containing the bromide paper or film, two lever systems the action of which is to be recorded, a light source, two signal magnets, a motor, and a reduction unit. The diagrammatic sketch shows the arrangements of the various parts.

The light source is placed in the center of the front wall (the side facing the subject). A six-volt, single filament single contact 32-c. p. automobile headlamp serves as the source of light. The lamp is enclosed in a brass housing 3¾″ in diameter and 4″ high. The top and bottom of the housing have hooded ventilating holes to carry off the heat generated. A regular

automobile coupling socket is used to hold the bulb and also enables an easy means of disconnecting the lamp circuit. The shaft of the coupling is about 2" long and its position is fixed by a set screw. Directly opposite the lamp socket a brass tube 1¾" in diameter extends a distance of 1". In this tube are located a pair of condensing lenses. By sliding the coupling back and forth the focus of these lenses may be altered to give the desired results. A 32-c. p. lamp under these conditions gives enough light to record on either bromide paper or film. In order to vary the intensity of the light for one or the other a variable resistance and a small ammeter are located in the rear wall of the apparatus and put in series with the lamp. We have found positive film the most satisfactory for ordinary purposes. It costs only slightly more than bromide paper and has the added advantage of being easily projected on a screen for detailed examination. With this film the lamp should draw in the neighborhood of 1 ampere. The current can be varied one way or another depending on the preference of the experimenter for light or dark records.

Directly in back of the tube containing the lenses the recorder is placed. The latter consists of a box 11½" x 6½" x 2¾"; in our apparatus the back, two ends, and the bottom were cast of aluminum but they can be made up of sheet aluminum just as well. In the lower corner of the figure a sketch of the inside of the recorder is illustrated. A spool 5" in diameter is located at each end to hold 200' of film. From the feed roll the film passes over a moving picture sprocket, then across a thin brass plate, over a motor driven sprocket to the take-up spool. On the lower end of the motor driven sprocket and on the lower end of the take-up spool are small brass pulleys which are connected with a spring belt which slips easily and serves to roll up the exposed film. A light friction plug is placed on the feed spool to keep the film taut at all times. The front of the recorder is a brass plate 11½" x 3¼" x 3/32" which is screwed to the casting. It is separated from the brass plate over which the film travels by about 1/64" which is sufficient to thread the film through without removing the front. In the center of the front plate, directly opposite the light source, is an adjustable hair slit. Over the slit a brass hood 1¾" in diameter and ½" long is fastened to keep out stray light. The end of the hood is covered except for a ⅛" vertical slit directly in front of the hair slit. The top of the recorder is a brass plate 1/16" thick which slides snugly under the overlapping edges, making the recorder entirely light proof. The top and bottom of the front plate serve as guides and travel in two steel rails 11½" x ¾" x ½" in which a groove has been milled. A third guide is attached to the rear wall of the recorder in the form of a piece of angle brass. This travels in a similar track and serves to hold the recorder firmly in place during the experiment.

Although the description is complicated the removal of the recorder and

the exposed film is extremely simple and requires only a very few minutes. The left side of the apparatus is attached with thumb-screws and these are quickly removed. The recorder slides out of its rails and can be removed in bright daylight without injury to the film. The recorder is carried to a dark room (with positive film a red lamp may be used), the cover slides off and the top of the take-up spool removed by means of a thumb screw. The exposed film is cut close to the motor driven sprocket to save re-threading. The exposed film is lifted from the spool and the new end fastened to the take-up spool by simply slipping the end in a slit in the spool. The top is replaced and the recorder is ready for the next experiment. If desired the entire 200′ length of film may be exposed without removing the recorder from the apparatus at all.

Between the hood of the recorder and the end of the projector tube is a gap of about ¼″. In this gap four wires move up and down casting shadows on to the hair slit aperture of the recorder. The graph consists of a record of the excursions of these four shadows. Since the only section of the film which is not exposed lies behind these wires, and since the film is moving at a constant rate, the curves are continuous. Two of these fine wires are attached to signal marker magnets. One jogs down every second, giving a time record, the other jogs upward at the giving of each stimulus word and the responses. The remaining two wires are attached to the ends of the levers which are activated by the manual pressures. The lever system is of the double fulcrum type to avoid displacement of the platform when the latter is pressed down. The entire lever system is constructed of 3/32″ aluminum tubing with a side wall of 1/100″. Since this tubing is extremely light the inertia of the system is negligible. Both of the fulcrums are of the jewel bearing type to reduce friction to a minimum. By changing the arm ratios either the smaller tremors or the large discoordinations can be brought into relief, depending on the experimenter's chief interest. For all around purposes we have found a lever stroke of ¾″ and a ratio of 4 to 3 very satisfactory.

When the two hands are used for the experiment, one passive and the other active, the platforms are placed close to the front wall on each side of the light source. This allows about 17″ between the platforms which is a comfortable distance for most adults. Experiments are now in progress to determine the discoordinating effect when two fingers of the same hand are used. For these experiments two smaller sets of levers have been constructed and used in like fashion, except that now the two platforms are 2½″ apart. A spring is attached from the upper arm of the levers to a screw extending down through the roof of the apparatus. The tension in the springs may be adjusted by means of the screws without disturbing the remainder of the apparatus. Experiments were also conducted with the same apparatus using tambours. The two tambours were placed on the

arms of a chair under which the apparatus had been placed. Tubes carried the impulses to the recording tambours which raised and lowered the fine wires whose excursions were then recorded. On the whole the lever system has proved to be far more satisfactory.

The entire apparatus is powered by a Bodine synchronous motor of the MSY-12R type to insure a constant speed. It is coupled to a reduction unit by a piece of heavy rubber tubing. The natural speed of 1800 r.p.m.'s is reduced to 3 r.p.m.'s in this unit. It seems better to couple on the high speed end in order that any torque in the coupling will be reduced in the ratio of 600 to 1 which makes it negligible. The motor is mounted in the following manner to absorb noise and vibration: The motor is bolted to a piece of ordinary shoe leather about ¼" thick; the leather is then glued to a piece of sponge rubber ¾" thick; the latter is again glued to another piece of shoe leather through which bolts have been inserted in the proper positions. The bottom leather is then bolted to the floor of the apparatus. This avoids any metal-to-metal contact and thereby abolishes the transmission of vibration to the apparatus as a whole. It is a sort of floating power arrangement.

The power is transmitted from the reduction unit to the recorder by means of a 5/16" shaft running horizontally from back to front, ⅞" from the floor. On the front end of the shaft is a spiral gear 1" in diameter. A similar gear extends from the bottom of the recorder on the same shaft that holds the motor driven sprocket. When the recorder is pushed in on the rails previously described, these two gears mesh and the power is transmitted to the sprocket which pulls the film through the recorder. In our present arrangement the film is pulled through at the rate of 3 mm. per second. This seems to be a satisfactory speed for ordinary purposes. Should greater or lesser speed be desired it can easily be changed by altering the gears in the reduction unit.

A commutator is attached to one of the shafts in the reduction unit travelling at the rate of 12 r.p.m.'s. Five points on the commutator furnish us with a contact every second which is recorded on the film by one of the signal marker magnets in series with the commutator. Both this magnet and the magnet recording the stimulus word and the responses are mounted on posts ¼" in diameter. Set screws make it possible to adjust the height of these magnets without touching the fine wires. We have found the records of both of these magnets to be most convenient when placed in the center of the film, although this may be only a matter of preference. The second timer record is not an absolute necessity since the film travels at a constant speed of 3 mm. per second, but it is extremely valuable for calibration purposes when the film is projected.

The stimulus and response magnet is controlled by a contrivance external to the apparatus. In our experiments we have been using an electronic

voice key not unlike some already described in the literature. A mechanical lip key or a manually operated key or switch is, however, quite satisfactory.

The inside of the box and all of the individual units are painted in dead black to prevent the reflection of stray light. If the outer box is fitted with a fair degree of accuracy no additional precautions against stray light need be taken. The hood over the hair slit of the recorder and the extension of the projector tube are very effective in preventing extraneous light from reaching the film.

This apparatus enables one to carry out all of the experiments that Luria has reported without the difficulties inherent in tambour systems or the mussiness and complications of the smoked paper technique. In addition it permits a record of indefinite length which can be readily projected on a screen; the graphs are not distorted, thereby facilitating quantitative measurements; it is at least as sensitive as Luria's apparatus; and it can be used with two fingers of the same hand as well as with fingers on the two hands. Furthermore, it is portable and convenient to handle and permits the experimenter to observe the expressions manifested by the subject during the experiment.

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BOOKS

C. U. Ariëns Kappers, G. C. Huber, & E. C. Crosby. *The Comparative Anatomy of the Nervous System of Vertebrates, Including Man.* Two volumes. New York: Macmillan, 1936. Pp. xvii+864; xi+981. $16.00.

This exhaustive treatment of the comparative anatomy of the vertebrates is more than a review of anatomical facts. There is a very good balance between the observed facts and the interpretation of them. For example, in describing the anatomical relationship between the motor and sensory areas of the cortex it is stated that, "This relationship, and to a certain degree, this overlapping of the sensory area into the motor region, is not surprising in view of the interdependence in function of the two regions, for an effectory center can be established only in relationship with the receptory which carries its impulses to it. Indeed, it may be established in such a way that the source of the entering impulse determines its position."

The preceding quotation is illustrative of the authors' treatment of the neurological material. The functional aspect has been stressed to a greater extent than in the usual neurology text, although references to the functional literature are by no means as complete as the bibliography of anatomical work. Rather than merely reviewing the field, the authors have attempted to answer why phylogenetic development has taken place in a particular direction. The distribution of neurones, the location of nuclei, the rise of new structures or the increased complexity of old structures are interpreted as due to the functional activity of the neurones. The principle of neurobiotaxis is the fundamental basis for these interpretations. In Chapter I some twenty pages are devoted to the formulation of the principle of neurobiotaxis and the evidence in favor of these "structural laws of the nervous system." It is unfortunate, however, that the authors neglected to discuss evidence which might support a mechanical theory of directional growth rather than the electrical theory supported by the authors.

It is neither possible nor advisable to describe the contents of this book in detail at this time, and, although they are merely indicative of the detailed treatment given the material, I shall only list the chapter headings so that the reader of this review may obtain some idea of the contents. I. The Evolution and Morphology of the Nervous Elements; II. The Comparative Anatomy of the Spinal Cord; III. The Medulla Oblongata; IV. The Lateral Line and Acoustic Systems; V. The Effectory System of the Midbrain and the Medulla; VI. The Coördinating Apparatus; VII. The Cerebellum; VIII. The Mesencephalon and the Diencephalon; IX. The Submammalian Telencephalon and the Mammalian Telencephalon

Exclusive of the Non-Olfactory Cortex; X. The Development of the Cortex in Mammals. Under the above main divisions the material is treated separately under Cyclostomes; Plagiostomes, Ganoids, and Teleosts; Amphibians; Reptiles; Birds; Mammals, including man. The text is profusely illustrated (710 illustrations), many of the illustrations being diagrammatic for the sake of clarity. At the end of each chapter is a résumé. These résumés are very illuminating, often containing conclusions and generalizations in addition to a summary of the content of the chapter to which it is appended.

The enormous bibliography, consisting of some 6000 citations, is extremely valuable. The references, usually at the end of each chapter, are arranged according to the vertebrate classes. Then too, many of the references made throughout the text are of the "evaluative" type, that is, not merely a name and a date for a certain piece of work, but an evaluation of the experimenter's work as compared to another author's findings if the subject is a controversial one.

The authors' point of view in the treatment of the material is well summarized by the last paragraph in the introduction: "No teleological explanation of the development of the nervous system was intended in the reference to the inner urge or development and expression characteristic of living matter. The nervous system has not developed during phylogeny with the brain of man as its fixed pattern or goal. Evolution as a whole has occurred without molding itself to a form established in anticipation of the progressive development. The ape-like progenitor of man did not have in mind the characteristic human form as a pattern which he should imitate. This may be expressed by saying that the development here is entelechic rather than teleologic, for each stage is an end within itself—not a step toward a well-known, predetermined goal."

The authors of this book are to be complimented for a work that surely will come to be regarded as a classic in its field. The clear, interesting style in which the book is written is something not usually accomplished in the presentation of such an enormous amount of detailed, highly technical information. It should prove invaluable as a sourcebook for the professional and non-professional neurologist whether he be interested in the anatomical or the functional aspect of the nervous system.

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